THE LANCET.

VOL. I.] LONDON, SATURDAY, JANUARY 12, 1839. [1838-39.

COURSE OF

LECTURES ON THE BLOOD,

AND ON

THE CHANGES WHICH IT UNDERGOES DURING DISEASE,

Delivered at the College of France in 1837-8,

BY

M. MAGENDIE.

LECTURE XV.

Anatomy of the capillary vessels.—Researches of M. Berres.—Experiments on the blood with various medicinal substances.—Action of tartrate of antimony and potass in inflammation.—Injection of oxalic acid into the veins.—Pathological phenomena produced by miasmata.—Division of the nervus vagus.

GENTLEMEN:—One of the grand errors of the physiologists of the last century, and, indeed, of those of our own days also, consists in their supposing the capillary circulation to be regulated by other laws than those of the circulation in the large trunks. We have examined together the chief phenomena occurring in one and the other systems, and with this result: that they differ only in respect of the explanations that have been given of them; but that they are, in reality, linked together and confounded in one common origin. Who will henceforth have the hardihood to call in question the all-powerful influence of the heart on the progression of the liquid through the capillary tubes? Have we not seen pulsations in the column of blood contained within them corresponding to each contraction of the central organ,—diminishing when the impulsion gets weak, augmenting when it becomes strong? When the passage of the blood through a large-sized artery is suddenly arrested the liquid continues to move in the end of the vessel next the capillaries; so, in like manner, the latter continue, for a few instants after the impulsion has ceased to reach them, to be traversed by currents of blood. There is no such thing as aspiration on the part of the venous radicles; there is no such thing as instinctive or spontaneous locomotion of the globules; there are no phenomena observable but such as are a simple consequence of the elastic retraction of the walls of the vessels; so that the heart by its contractions, and the vessels by their elastic retraction, effect the circulation of the blood in the normal state. When both these causes act the movement is jerking; when the action of the first is neutralised, the jerking is transformed into a continuous movement, and this persists until the elasticity of the membranous tubes is thoroughly exhausted, after which all movement is at an end. To those persons who would ask in what respect the hydrodynamic phenomena of the large and small vessels resemble each other, I should, in reply, put the converse question: I should inquire in what they differ; in truth, I see none but points of similarity to be established, and no important distinction to be admitted, between them.

One of the first things to be done by any one attempting to draw up a physiological and anatomical history of the capillaries is, to separate them into distinct groups, each referrible to a certain type. The various points in the history of these vessels, which I have laid before you, are very easily verified in the tissues in which they run in an isolated manner; thus, the mesentery of young animals, the bladder of small rats, distended with urine (as it is at the moment of birth) and the interdigital spaces of the web of a frog, are well adapted for microscopic investigation. But though every capillary tube may be abstractedly looked on as a simple vessel commencing in an artery and ending in a vein, still there are differences to be pointed out as respects the mode of their arrangement in the tissues of which they concur in forming the parenchyma. Here commences a new series of difficulties. I have explained the advantages and disadvantages of adopting injection as your mode of facilitating the study of the capillaries; supposing even that you have, by patience and skill, acquired the knack of introducing the colouring liquid into those vessels alone that are traversed by blood during life, and into none others, still you will not have any very distinct notion of the disposition of the capillary rete. To

ensure anything like an accurate view of the system, you ought to be able to graduate the force employed in driving in the injection according to the resistance of the vascular walls, and to compose a mixture insusceptible of penetrating into the pores of membranes. Now, we are unfortunately far from having reached such a degree of practical skill as this; extravasation almost always occurs, either from laceration of the vessels or from imbibition; both those causes, indeed, usually influence the result; so that, instead of linear canals, we get a succession of minute cylinders, interrupted by multitudes of swellings and ruptures.

I know not to what extent these difficulties have been triumphed over by M. Berres, a German anatomist, who has recently published a work entitled "De Anatomiâ Partium Microscopicarum Corporis Humani," professing to contain a detailed history of the capillary vessels. This production is one of the most complete that has yet appeared; the text is illustrated by an atlas of plates, in which the various modes of arrangement and of anastomoses of the capillary tubes, as well as the microscope used by the author, are carefully represented. In spite of all the labour M. Berres appears to have bestowed on his researches, I should be far from affirming that his book is a perfect one, and that henceforth this section of the science may be looked upon as fully understood. I have already, I believe, pointed out some omissions on his part; but as to pass his classification thoroughly in review would be rather a subject of curiosity than of really instructive study, and as what we want most especially is to have a general idea of the grand divisions of the vascular system, I shall content myself with enumerating the sixteen classes he has established:—

Class

I. *Plexus vasculosus linealis cruciatus.*
II. *Plexus vasculosus undulatus fortior.*
III. *Plexus vasculosus undulatus tenuis.*
IV. *Plexus vasculosus linealis pectinatus.*
V. *Plexus vasculosus, erectilis linealis.*
VI. *Vasa arcuata, plexus vasculosi dentritici.*
VII. *Plexus vasculosus longitudinalis solidus.*
VIII. *Plexus vasculosus dentriticus.*
IX. *Plexus vasculosus longitudinalis reticularis.*
X. *Plexus vasculosus longitudinalis cellularis.*
XI. *Plexus vasculosus excentricus longitudinalis ramosus.*
XII. *Vasa tenuissima, plexus vasculosi linealis cruciati.*
XIII. *Plexus vasculosus maculoso longitudinalis.*
XIV. *Plexus vasculosus excentricus radiatus.*
XV. *Plexus vasculosus excentricus sarmentosus involvens.*
XVI. *Plexus vasculosus pencilliformis erectilis.*

You perceive, Gentlemen, that this classification is somewhat arbitrary; the denomination of each group indicates to a certain extent, it is true, the observed arrangement of the vessels; but it may be fairly asked, whether these different aspects do not rather depend on the kind of sections made through the tissues, than on a real difference in the disposition of the capillary tubes. Still it is right to add, that I had an opportunity, the other day, of recognising the variety indicated in class XIV. I was examining an injected kidney, and I fancied I perfectly distinguished the branches directed from the centre to the periphery and terminating in a flexuous fasciculus, with radiated meshes; there was a lobule, furrowed with a sort of groove, interposed between each pair of vascular branches. I have also verified the correctness of the description of some other groups; so that, on the whole, I feel some confidence in the exactitude of the general results announced in the book.

Further researches on the intimate structure of the capillary vessels are, however, called for, before we are entitled to generalise, in an absolute manner, what I have told you concerning the progress of the blood in their cavity. We should be enabled to compare the principal apparatuses with each other; the organ that secretes the urine and that forming the saliva cannot, in respect of their vascular arrangement, resemble those which elaborate the bile and pancreatic juice. Each tissue and parenchyma has its special mechanism, and its texture is thereby adapted to the functions devolving on it. The walls of the vessels themselves, in all probability, present a multitude of different degrees of porosity, according as they are destined to allow of the passage of this or that material from the blood. There is a vast field for inquiry here; the various secretions, mucous, serous, glandular, nutrition itself, all of them phenomena as admirable as complex, cannot be thoroughly understood unless we first obtain a profound knowledge of the organisation of the tissues, membranes, and parenchymata.

But, Gentlemen, all our attention must not be lavished on the solids; if these merit much of our consideration, there is a still more important subject requiring investigation at our hands. How can we reasonably hope to modify the condition and functions of our tissues if we neglect the state of the liquids? Do not these contain, in the state of health, the materials to repair the losses of our organs? in the state of disease, those fitted to remedy their lesion? The liquids are the sole route whereby medicinal substances can enter the system; the sole source

from which our organs derive vigour and life. We have made real progress since the beginning of last session; what I then only suspected has since been rendered a matter of certainty. Some former experiments gave me a hint as to the disorders induced in the organism by alterations of the blood; and those we have made of late have placed this fundamentally important doctrine beyond all reach of being contested.

I have, since we last met, examined, under the microscope, the different mixtures of acid and blood then shown you; I have a few remarks to make respecting the results arrived at. As regards the chemical analysis, M. Frémy has undertaken that part of the investigation, and when he has concluded it, you shall be made acquainted with the details. All the acids with which we experimented have given rise to very nearly the same phenomena; I sought in vain for any traces of fibrine in the various solutions. The liquid acted on with acetic acid is almost transparent; some nebulous flocculi and very delicate filaments only are observable here and there. In the mixture made with lactic acid I succeeded in making out some delicate particles in suspension, and globules of a particular sort, forming small masses by their union; the colouring matter and fibrine had totally disappeared. In the vessel containing hydrochloric acid, I discovered some small bodies of unusual form held in suspension, some of them rectilinear, others curved in the shape of an S, others in that of a crescent; there were still some traces of globules to be seen, but in all probability in a state of disorganisation, for they were larger than those of normal blood. Solution was least perfect in the mixture containing tartaric acid; here were to be seen large coloured globules of various dimensions; close to these, others of the same form, but colourless; next, some much smaller, and finally, irregular corpuscula. It remains to be ascertained whether these were the detritus of globules or of fibrine; upon this point I can scarcely hazard a conjecture. The colourless globules resembled the large white globules which are to be seen in blood immediately after it has been withdrawn from the vessels. In order to procure them we prick the finger slightly with a pin, scrape up the drop of blood discharged on a plate of glass and examine it under the microscope. Now, no corpuscula of this kind are to be seen in blood in circulation, and it would consequently be very curious if tartaric acid possessed the property of rendering these globules apparent. All the other substances tried had entirely liquefied the blood; no trace either of globules or fibrine was perceptible in the mixtures made with them.

I now return to our experiments: as you no doubt expect, they will have reference to medicinal substances; for it is a matter of primary importance to ascertain, accurately, the kind of effect produced by these substances on a fluid which, in its turn, acts on all the tissues of the economy.

1st Experiment.—Carbonate of soda.

Here is a vessel containing 60 centilitres of water, two grammes of soda, and five centilitres of blood; the liquidity of the mixture shows clearly how this substance must act when introduced into the circulation.

2nd Experiment.—Bicarbonate of soda.

Bicarbonate of soda, water, and blood have been mixed in the same proportion as in the last experiment, in this vessel. No coagulation has occurred here either; there are even some crystals of the salt still undissolved, showing that the entire quantity thrown into the mixture has not been required to effect the liquefaction of the blood employed. The liquid is of a bright red hue. This substance, which enters in a very considerable proportion into the composition of the Vichy waters, has a peculiar action on the urine, communicating its own alkaline character to that fluid; it is employed in gout, articular rheumatism, and a variety of other affections, in doses of from one to four drachms daily. This experiment confirms what I before told you respecting the property possessed by the salt in question, of giving the blood an arterial colour, at the same time that it prevents it from coagulating.

3rd Experiment.—Carbonate of ammonia.

This salt has not dissolved perfectly either; in the midst of the liquid is a small and very delicate clot. We cannot, from this experiment, form any decided opinion on the action of this substance.

4th Experiment.—Subcarbonate of potassa.

The subcarbonate of potassa is, as it were, a succedaneum of the bicarbonate of soda; it is employed under the same circumstances as the latter salt, but more especially in cases of phosphate of lime gravel, a very common affection in Burgundy. It dissolves the blood, but, herein differing from the salt of soda, it gives it a black colour.

5th Experiment.—Sulphate of potassa.

The sulphate of potassa has furnished a precipitate which I believe to be formed of albumen, or globules; I must endeavour to ascertain this with greater certainty.

6th Experiment.—Chloride of lime.

The effect produced by this substance is hardly appreciable; however there is an appearance of commencing coagulation of the fibrine.

7th Experiment.—Chloruretted water.

Here there is no coagulation; and the mixture is tinged black.

8th Experiment.—Sulphate of iron.

In this instance very evident chemical reaction has taken place; there is an abundant

precipitate of albumen; no conclusion can be drawn from this experiment respecting the influence of the salt employed on the coagulation of the fibrine.

9th Experiment.—Alum.

Alum, which is, chemically, a sulphate of alumina and potassa, is employed externally, under the impression that it acts as a very powerful astringent, yet you see that not a trace of coagulum is to be detected here.

10th Experiment.—Bichloride of mercury.

This salt has produced a very strange effect; in the upper part of the vessel are globules in a state of solution, in the lower a combination of the chloride with albumen. The result of this experiment, therefore, very fully proves the excellence of the ordinary habit of employing albumen as an antidote for corrosive sublimate.

11th Experiment.—Acetate of lead.

The acetate of lead has precipitated the albumen without acting on the fibrine.

12th Experiment. — Ioduretted hydriodate of potassa.

We now come to a substance from the use of which very admirable effects are obtained in cases of chronic diseases of the bones and articulations, of scrofula, and various other complaints, till of late pronounced to be incurable,—I mean the hydriodate of potassa. You have all of you heard of the advantageous results of the exhibition of this medicine in cases of white swelling, old anchylosis, coxalgia, &c.; it is, indeed, one of the substances which I employ, with greatest success, in a variety of disorders. It has here produced a very remarkable effect; coagulation has occurred, but whether it be the albumen or fibrine which has solidified I am unable to determine at this moment. I dilute the mixture with water, and forthwith, as you perceive, a large number of brick-red flocculi appear. This experiment is worth the trouble of further inquiry, and it shall, therefore, be repeated.

13th Experiment. — Ferrated cyanuret of potassa.

The cyanuret of iron and potassa has completely liquefied the mixture.

14th Experiment.—Arsenite of potassa.

As for the arsenite of potassa, one of our most active medicines, it has so completely decomposed the liquid that it is impossible to say what name should be given to the matter you see in the vessel; all that is quite certain is, that not a shadow of coagulation is to be discovered.

15th Experiment.—Nitrate of silver.

Four grammes, or one drachm, of nitrate of silver, 60 centilitres of water, and two of blood, have been mixed here; a very distinct nebulous clot has formed. This salt is not much used internally, but its external applications are numerous and diversified in their intent; you perceive that it modifies the coagulation of the blood, but does not prevent it from taking place.

16th Experiment.—Alcohol.

Here are 10 centilitres of alcohol, 60 of water, and one of blood; the modification in the coagulation is only trifling, consequently this substance is not pernicious when taken in such quantities only, a circumstance which affords me much gratification for the sake of its very numerous consumers.

17th Experiment.—Phosphate of soda.

This salt has furnished a remarkable coagulum; you observe that it does not, in a single particular, present the characters of a clot of ordinary blood; there are undissolved crystals at the bottom of the vessel.

18th Experiment.—Tartrate of antimony and potassa.

I have already made some researches on the subject of tartar emetic, but they were not directed to the point now under consideration. This salt is much used in pneumonia, with a view to its acting on the respiratory organs, in rheumatism, &c.; some practitioners administer it in combination with the sulphate of quinine, in certain intermittent fevers. I have here put two grains of tartarised antimony into a mixture of six centilitres of water and one of blood; its action has borne principally on the globules, which are dissolved, and have tinged the liquid red; however, a nebulous clot has also formed. I am inclined to doubt, from the effects produced by injecting this salt into the blood, that it can produce favourable results by its immediate introduction into that fluid. Thus I injected a small syringeful of a solution of one drachm of tartarised antimony, in four ounces of distilled water, into the jugular of a dog. The animal almost immediately gave signs of being under the influence of the salt by making movements of deglutition; such movements always precede the act of vomiting, and, indeed, seem necessary for its accomplishment. It is not, as has been advanced, through contraction of the muscular fibres of the stomach, that the contents of that viscus are expelled during the process of vomiting; far from undergoing retraction while that process is going forward, the organ actually expands, and it is in order to facilitate and promote this expansion that the animal intuitively swallows a certain quantity of air. This fact has not escaped the notice of the sailors on board our packets; when they see a passenger seized with involuntary movements of deglutition, they immediately say that he is going to be sea-sick. Once the stomach is distended with air, the abdominal muscles and diaphragm compress its walls, and expel the matters contained within it. These results,

experimentally arrived at, led me further to the important discovery, in a therapeutical point of view, that the action of tartar emetic does not bear directly on the gastric mucous membrane. I removed the stomach of a dog, and substituted a pig's bladder in its room, and found that an injection of tartarised antimony into the veins induced nausea and vomiting as before. Be these matters as they may, however, the important point for present consideration is, that vomiting was soon followed by dyspnœa, cough, and fever; all these symptoms indicated the existence of a serious lesion of the respiratory apparatus, and death supervened in the midst of the various phenomena proper to pneumonia. After death the lung appeared somewhat altered in colour, it was grayish; it had, indeed, the look of a lung infiltrated with a little pus, but this was not really the case; consequently tartar emetic does not, like mercury, act simply in determining phenomena of obstruction; it attacks the blood chemically, decomposing some of its elements, and to this change in its composition were, in great measure, due the symptoms and cadaveric phenomena observed in the case adverted to. The lungs, however, were not the only organs affected, —there were dark red patches and vascular arborisations visible throughout the entire extent of the intestinal canal, indicating obstruction in the abdominal circulation. I in vain strove to push an injection through the mesenteric arteries; the liquid did not return by the corresponding veins. The obstruction of the capillary tubes was here, too, the starting point of the lesions produced; the *gastro-enteritis* I detected was no more than a mechanical effect of the exhibition of blood through the walls of the vessels, and its consequent extravasation into the surrounding tissues.

These chemical effects of tartar emetic appear the more extraordinary when we reflect on the confidence placed in this salt as a remedial agent in certain disorders. The author of the magnificent work on Mediate Auscultation has filled several pages in expatiating on the advantages he had derived from its administration, especially in the disorders already named,—pneumonia and rheumatism. Now, it happened that I was designated to succeed Laennec at the Necker Hospital; and, out of respect to that illustrious observer, I continued the exhibition of the same preparations of antimony, and in the same affections as he was in the habit of employing them. But I must confess the results were not such as I had hoped to see them; though I had the same house-surgeon and the same apothecary as my predecessor; though everything, in short, remained precisely as he had left it, I could not satisfy myself that any very notable modification in the progress and duration of those affections was produced by the use of the vaunted drug. I consequently, after a few weeks' trial, ceased altogether to employ it. You must be careful, Gentlemen, about forming your opinion too precipitately respecting the efficaciousness of a medicine because it has apparently produced a few successful results. Who can assure you that the patient would not have recovered quite as well if you had not employed it? Rheumatism, for example, yields to bleeding, or to tartar emetic, to every imaginable kind of treatment,—yields, above all, to rest and diluents; in my hospital practice I never have recourse to the lancet, to tartarised antimony, or to leeches in the treatment of this affection, and yet I have no hesitation in affirming that all the cases of rheumatism I have treated have terminated favourably.

19*th Experiment.*—Cinchonine.

This alkaloid exists in cinchona bark in conjunction with another substance belonging to the same class, quinine. One grain of it added to this mixture has given rise to the formation of one of the most delicate clots I have ever seen; it resembles vegetable jellies before they have become completely solid by the progress of refrigeration.

20*th Experiment.*—Sulphate of quinine.

This substance has also formed an almost invisible clot; possibly, however, the formation of the coagulum in this instance depends on the presence of the sulphuric acid, which we are obliged to employ in order to produce a soluble material. We must vary the experiment in some way, in order to ascertain the nature of the action of the alkaloid on the blood with greater precision.

21*st Experiment.*—Decoction of digitalis.

The decoction of foxglove, well known to you for its sedative effects on the contractions of the heart, has prevented coagulation from occurring.

Here then, Gentlemen, you have a series of facts regarding a question of high interest, all of them completely new; these are so far mere trials, but we will not stop here; when we shall have exhausted the list of substances worthy our attention, we will repeat such experiments as may not have given us complete satisfaction. Once our review has terminated, we shall, I trust, have acquired some new and solid information on the *modus operandi* of medicines, one of the most obscure points in the science. So far, you will observe that we have met with few therapeutic agents capable of increasing the coagulability of the fibrine; for a solitary substance possessed of that power, we have found several capable of liquefying it. Nevertheless, we must hope to be more successful in this way in our ulterior researches; they will doubtless afford us compensation for our disappointment. But even if our expectations in this respect fail altogether to be realised, we

shall at least have the satisfaction of feeling that we have, by our results, shown the necessity of expunging from medical formularies a number of substances which are capable of inducing disorder in the economy. This single result is worthy of attention, as it appears to me; for our art has been instituted for the relief of the sufferings of our fellows, and not for the purpose of allowing us to make a vain parade of factitious science; to heap drug on drug without caring for the effects they produce. For my own part, I care little for the learned prescriptions in which the majority of practitioners delight; the mysterious dignity of their composition always seems to me calculated to throw chaff in the eyes of the vulgar, and rather to enhance the merit of the physician than really to effect the recovery of the patient. Though my amour-propre may often suffer thereby, I prefer infinitely to abstain from prescribing altogether, to ordering drugs respecting the effects of which I am ignorant. Far from discouraging you, Gentlemen, these considerations should animate you with fresh zeal; they point you out a vast and untrodden field for research. If you never lose sight of the precepts taught you from this chair—precepts of which I have invariably endeavoured to give you in my own person the practical illustration—if you are guided in your researches by a spirit of the strictest exactitude, rejecting all that is not proved by experiment and observation, each of you may hope one day to occupy an honourable place among the benefactors of humanity.

You remember that I introduced seven grains of oxalic acid into the jugular vein of a dog, at the close of my last lecture. The lateness of the hour prevented us from waiting for the results of the introduction of the poison to manifest themselves; I proceed to inform you of what has occurred since our separation. The animal was seized with dyspnœa soon after the operation; his condition grew more and more desperate every hour, and he expired the following morning. Here is his body: judging from the effects of oxalic acid on the blood in glass vessels, we must expect to find the blood in his body perfectly liquid. But do not let us anticipate,—I remove the anterior wall of the thorax, but the lungs do not collapse in consequence. I open the pulmonary artery; the blood flowing from it is liquid and brownish, just as it would have been had it been acted on with sulphuric acid; and, therefore, the conclusion is inevitable, that oxalic acid produces the same effects on the blood while circulating in the vessels, as when removed from them. This is an important fact; it confirms me more strongly than ever in the opinion that the vitality of the blood and of the walls of the vessels neither prevents nor even modifies the chemical action exercised by a variety of substances on that fluid. As for the lesions of the pulmonary organs, it is enough, for the present, to say that the cells are distended with effused blood; that they were thus rendered unfit for supporting respiration, and that death was produced by asphyxia. The autopsy will be proceeded with after the lecture, and such of you as are anxious to ascertain for yourselves the nature of any remaining lesions produced by the poison, may be present at it.

I now proceed to the autopsy of another animal, who died yesterday from the effects of an injection of putrid water into the veins; although he only survived the operation a single day, we should, in addition to finding the blood blackish, liquid, and clammy, detect morbid alterations in the intestinal mucous membrane; those existing in the lung are, on the other hand, likely to be unimportant. I almost venture on predicting the pathological results of this sort of experiments, for they have hitherto been invariably uniform. I may here allude to an opinion, which has been adopted without the least foundation, respecting the cause of the cadaveric rigidity remarked in all subjects dying of yellow fever, the cholera, and other affections originating in the same source, miasmatic infection. It has been stated that the phenomenon depends on the solidification of the fibrine. Now, I appeal to the evidence of your senses; tell me if, in diseases of the description alluded to, as well as in those I produce in animals, the blood be not fluid and its fibrine liquefied? Here, then, is another erroneous notion to be rectified. I open the thorax of the animal, and you perceive, in truth, that the lungs present no apparent lesion; they have preserved their normal elasticity, and in nowise resemble those of the animal poisoned with oxalic acid. Here is one of my anticipations realised; and, further, it would appear to result from these experiments that the viscosity of the blood does not prevent it from traversing the capillaries of the lung, whereas the contrary is the case with the abdominal vessels of that class. I open the pericardium: the heart is flabby and collapsed, the blood in the right cavities liquid, black, and viscous; the left ventricle is perfectly empty. Passing to the abdomen we find the intestines black coloured, *inflamed*, according to the current phrase; transudation has taken place on the internal surface, and is partly constituted by the colouring matter of the blood; this appearance fully accounts for the bloody stools of the dysenteric flux, such as this animal laboured under. We are thus enabled by our experiments to point out the mechanism of the most serious pathological conditions. Where are the phlogistic authors that can base their ephemeral theories on such conclusive and irrevocable facts as these?

We have another animal to examine; the

mode of death in this case will supply as interesting materials for consideration as in the others. About six weeks ago I divided the right vagus nerve in this animal; his respiration became slightly laborious after the operation, but after the lapse of a few days he had recovered from its effects so completely that he presented no abnormal symptom. The day before yesterday I divided the nerve on the left side, as well as the cellular band that had formed between the divisions of that previously cut. I was desirous by this experiment to ascertain if the nervous action prevented the coagulation of the fibrine. The animal died a few hours after the second section. I have just opened the axillary vein; observe that the blood flowing from thence is as fluid as it is possible for it to be; the lung is evidently diseased; one of the lobes of the right organ is entirely *splenified*, to use the expression of certain pathologists. It is possible this lesion was developed under the influence of the section of the eighth nerve; still one of the left lobes, the nerve of which was only divided the day before yesterday, is almost as profoundly altered in character as the right. The blood distending the organ is also quite fluid. The experiment must be repeated several times, before we can feel certain as to the effects produced by the section of the pneumogastric nerves on the important phenomenon of the coagulation of the blood.

REMARKS ON THE PLAGUE IN REPLY TO DR. BOWRING.

By HENRY NORRINGTON, *Esq., Axminster.*

To the Editor of THE LANCET.

SIR:—Having seen in THE LANCET for Nov. 24th, an abbreviation of Dr. Bowring's "Observations on Oriental Plague and the Quarantine Laws," I send you a few remarks on those observations as there reported; if you deem them worthy of insertion, they are at your service. I was in Egypt and Palestine during part of the years 1833 and 35, and the whole of 1834; and whilst in the East my attention was particularly directed to the plague, to which I nearly fell a victim at Jerusalem in 1834.

Every one who has visited the quarantine establishments of the East must, most certainly (whatever be his opinion as to the contagion or non-contagion of the plague), declare them to be the most infamous establishments of the kind in the world. I do believe that, if a set of demons had put their heads together for the purpose of contriving a place for the creation and diffusion of plague, and disease of every kind, they could not have formed one more fitted for their design than the lazaretto of Alexandria in 1833. None of the lazarettos of Egypt or Syria are better than that of Alexandria; some are worse. All are disgustingly filthy, and no attention whatever is paid to the wants of the unhappy inmates. By a little bribery, persons from the towns are admitted by the guards into the lazarettos, and allowed to return, even after personal contact with the suspected individuals; and a Jew assured me that, for three dollars, he had been allowed to leave the lazaretto of Alexandria, although he had come from Smyrna, where the plague was raging. I firmly believe the plague to be contagious; but I also firmly believe the quarantine establishments of the Levant are worse than useless; that they very often cause the evil they were intended to prevent. Malta has a good lazaretto, and though the delay there is inconvenient and expensive, yet I had no occasion to complain of extortion or filth. I fully agree, then, with Dr. Bowring, that the lazarettos of the Levant should either be abolished or entirely remodelled; but to many other remarks made by the learned Doctor I cannot yield my assent. Some of Dr. Bowring's assertions, as given in THE LANCET, are certainly mistakes, and in some places he contradicts himself. For example, in page 343 of THE LANCET, he says, that "the supporters of quarantines have, at starting, to grapple with one astounding fact, namely, that among the Mahomedan population of the East, where the plague commits its greatest ravages, and who see and treat it perpetually, nobody, with the very rarest exceptions, believes in its contagious character." In this passage the Doctor tells us, that the Mahomedan population of the East do not believe in the contagious character of the plague. Yet, in the next column of the same page we are told, that the doctrine of contagion comes from the East; and a few lines below is the following passage:—"The extent of credulity in the East, respecting contagion, may be estimated by the fact, that when the Turks wanted to destroy the Austrians, they inserted the matter from the buboes of the plague behind the ears of the pigs which they sent to the market at Semlin, by which the butchers were to catch the plague, &c." Here, then, there is great credulity in the East about contagion, and that, too, among the Turks, who are the Mahomedan population of the East, where, according to the Doctor's first statement, "nobody, with the very rarest exceptions, believes in its contagious character." Do the Mahomedans, then, believe the plague to be contagious, or do they not? Most certainly they do believe it contagious; at least, I have never met with one who did not. Dr. Bowring says (LANCET, page 343), "They vaccinate willingly, they seek with extraordinary eagerness and confidence the aid of medical men, &c. &c., and if they believed in the

contagion of plague, they would fly from the calamity."

I do not think that this conclusion can be drawn by Dr. Bowring from his premises. What, because Mahomedans vaccinate willingly, and seek the aid of medical men with confidence; because they do not abandon their relatives when attacked by the plague, and leave them to die without assistance, does it, therefore, necessarily follow that they believe the plague not to be contagious? I think it is certainly a *non sequitur*. To have proved his proposition, Dr. Bowring should have demonstrated to us that the Mahomedans do always, or, at least, generally, fly from diseases which they think contagious, and leave their parents and children unaided; then, in that case, the *fact of their not flying* from the plague would prove something. But is Dr. Bowring prepared to assert, that the Mahomedans *generally* fly from contagious diseases? I should think not. They believe the small-pox to be contagious; they vaccinate, says the Doctor, willingly; they have a great dread of the disease, and try all they can to avert the calamity; yet, when it does seize upon their relatives, they do not desert them. Certain precautions are, however, taken among the respectable classes; no one, generally, approaches within a certain distance of the sick person but an old female slave who has before had the disease. With the poor it is different; crowded together in their miserable habitations, it is almost impossible for them to take any precautions, and the disease spreads among them with frightful rapidity. It is just the same with the plague; when an individual is attacked they do not desert the house; the relatives of the infected person enter the room, and speak to him, but still it very rarely happens that they come into personal contact with him, everything that requires contact being performed by one or two appointed persons. There is no difficulty either in finding individuals to undertake this task. Persons who have had the plague before are generally chosen, and if none of these are to be found, some of the household slaves or relatives of the infected person are always willing to attend, and perform every kind office with the greatest assiduity; for the Mahomedans are kind-hearted and charitable and benevolent, and though, on some occasions, they shed blood with great recklessness, yet sickness and poverty never plead to them in vain, but always meet with commiseration and assistance. Among the respectable Mahomedan families with whom I was acquainted, I always found that none, except those who were necessary to the infected persons, came into contact with them. Cases in which this precaution had been neglected were sometimes told me; but always spoken of as acts of rashness. A diamond merchant of Cairo told me that his brother had caught it by embracing his wife a day or two before her death from the same disease. I am sure that my informant believed in the contagion of the plague, for he called his brother "Madjnoon Kbir," a great fool, because he had touched his wife,

Dr. Bowring says that Clot Bey has irresistible evidence of the non-contagiousness of the plague; if so, I hope that this evidence will be produced. Anything coming from Clot Bey deserves attention; he is a man of talent and experience, and were there three or four more such Beys in Egypt, we should soon see arising in that country a respectable body of surgeons and physicians. It seems, too, that Gaetano Bey entertains opinions similar to those of Clot. The opinions of Signor Gaetano are not considered of any weight in Egypt, and it was never known in that country what services he had performed to merit for him the title he possesses. Indeed, with the exception of Clot and two or three English practitioners, most of the individuals calling calling themselves surgeons and physicians in Egypt, are Italians, who come over from Genoa and Leghorn, many of whom have never received any medical education at all; and some (the best portion of them) are persons who left their own country for political crimes whilst they were yet students. I do not mean that Gaetano Bey is not a physician, but he is certainly not an eminent physician. Many of these gentlemen call out that the plague is not contagious, but I know many instances in which they have fled at the approach of that calamity. Dr. Bowring seems to lay some stress upon the facts that Clot Bey inoculated himself twice, and that Dr. Hepitis made several attempts to give himself the plague, and that both failed. But Dr. Bowring must surely be aware that the effect of contagion depends very much on the habit and state of the person who is exposed to it, and on many concurring circumstances, if any of which be wanting the disease will not be communicated.

The small-pox is a very contagious disease, yet some persons are incapable of having it; others do not receive it though repeatedly inoculated, and yet receive it afterwards, by merely approaching those labouring under it.—(See Thomas "On Inoculation," p. 259). It is the same with the plague, it requires a certain predisposition before its contagion can take effect. The mere fact, then, of Clot and others escaping, after contact and inoculation, proves nothing more than that those individuals were not then in a state to be affected by the plague. Similar instances are to be met with in the history of every other contagious disorder. On the other hand, there are numerous instances of persons who have received the

plague from inoculation, and fallen victims to their temerity. Sir Robert Wilson, in his "History of the Expedition to Egypt," says, that when the plague raged at Rosetta, Dr. Whyte inoculated himself with pus taken from the buboes of an infected person. The attempt failed twice, the third proved fatal in three days after the symptoms showed themselves. Sonnini tells us that a Russian surgeon, who was prisoner at Constantinople with a number of his countrymen, inoculated them for the plague, and, by so doing, killed two hundred of them, and would probably have killed the rest, had he not inoculated himself, and fallen the victim of his own speculation.

Giovanni Marotti, a young Florentine surgeon, with whom I was well acquainted, inoculated himself at Cairo, with matter from the buboes, and in four days afterwards was seized with the plague, from which, however, he recovered, I need not say completely cured of his ideas of non-contagion. So much for inoculation; as to contact, I have many facts which I think will prove that the disease is communicated by it.

While I was at Jerusalem in 1834, the plague broke out in the Franciscan convent, which was then my abode. In every case of plague which occurred there, its progress could be traced from individual to individual, and satisfactorily accounted for. The plague was introduced into the convent in the following manner:—The infirmarian of the convent was in the habit of paying charitable visits to the sick Christians in Jerusalem, and supplying them with any simple remedies which their cases might require. He was informed one day that two or three persons in a Greek family, which had arrived a short time before from Smyrna, were ill. The infirmarian visited them, and about four days after was seized with intense pain in the head, in the groin, and in the axillæ, and with all the other symptoms of plague. The day after he was removed to the infirmary, the mother of the family he had visited died of plague. The poor infirmarian died on the third day after the disease had declared itself. The next person who fell a victim to the plague was a priest, who was accustomed to use the same vestments at mass which were worn by the infirmarian. A lay brother, who acted as second infirmarian, and attended the two former individuals during their illness, was attacked simultaneously with three other friars, all of whom had been in personal contact with the first infirmarian before it was discovered that he had the plague. One of these three had been rubbed by him in the back for lumbago; in fact, I can trace the progress of the plague through the whole three-and-twenty who died in the convent, but I have already occupied too much of your attention. I suffered intensely from the plague for nine days, and it was nearly a month before my strength began to return. I know perfectly well the individual from whom I received the disease, and though very much inclined, on my first arrival in the East, to doubt the fact of contagion in the plague, my own personal experience and the cases that came under my observation, have completely changed my opinions on the subject.

In page 345 of THE LANCET, Dr. Bowring says:—"Mothers frequently die of the disease, without communicating it to their sucking children," and therefore I suppose we are to conclude that the plague is not contagious. But every one at all conversant with contagious diseases, knows that children are not so susceptible of them as adults. Dr. Thomas says, "I have observed that infants do not readily take fevers, and every physician who attends lying-in hospitals must not only have known many infants suckled without injury through the whole stage of bad fevers, from which their mothers have recovered, but also in other instances sucking greedily within an hour or two of their mother's death."—(Thomas, p. 62, 63.) Every one who has observed attentively the progress of plague cannot have failed to remark, that women and children escape in a wonderful manner. Joao Coelho, a Portuguese captain, who was for many years a prisoner amongst the Moors, in an account of his sufferings published a third time at Coimbra, in 1710, says, "It is wonderful how these infidel women and their children escape the pest. They wait upon their husbands, even when about to die, they embrace them, and make their children receive the last kisses of their fathers, yet strange is it to say, very few are taken ill." Thomas, in page 289, on the Plague, says, "Women, young people, and infants at the breast have been observed, in general, to resist infection more than robust men." Dr. Bowring seems also to imagine that, because the garments of those who have died from the plague are sold, and the individuals who purchase them are not infected, therefore these garments are incapable of conveying infection. I will at another time prove that this fact, which seems so conclusive to the Doctor, is in perfect accordance with the laws of contagion; that the infection is conveyed by the garments thus sold, and that when the proper season and circumstances occur it shows itself in all its natural malignity.

I will now conclude for the present, with a remark or two on a most curious assertion made by Dr. Bowring (in page 346 of THE LANCET). He says "Frequently do the Arabs die on the bales, and the pus from the bubo is often packed up for our market." With how many instances of Arabs dying on the bales is the Doctor acquainted? Not many, I think; and even should it ever have

happened that an Arab has died on a bale of cotton, it surely does not follow that the pus from his buboes has been "packed up for our market." Besides, should an Arab be so perverse as to choose a bale of cotton destined for us as his death-bed, rather than any other place, it is more than probable that his buboes will have no pus to "pack up for our market." Baron Larrey, in his "Memoirs of Military Surgery," speaking of the plague, says, "That when the buboes terminate by suppuration they seem to produce a favourable crisis." In another place he says, "If the buboes did not suppurate, all the symptoms made a rapid progress, and the patients died by the third or fifth day."

So there is every chance, that if the Arab does die on our bales of cotton, he will have no pus to pack up, since death generally happens from the want of it; and if he has pus, there is every chance that he will not die on our bales at all, since suppuration produces a favourable crisis. It only remains for me to add that I am not a medical man, and my only object in forwarding to you these observations is to give, through the medium of your widely circulated Journal, any facts that may tend to throw light on a subject of such importance as the Oriental plague. I remain, Sir, your obedient servant,

HENRY NORRINGTON.

Terrace Lodge, Axminster,
Dec. 29, 1838.

P.S. I could not notice Dr. B.'s observation before, THE LANCET for NOV. 24 was only put into my hands two days ago.

A KNOWLEDGE OF MEDICINE AN ESSENTIAL REQUISITE IN A CORONER.

To the Editor of THE LANCET.

SIR:—Permit me, through the medium of your extensively circulated periodical, to offer a few remarks in connection with an often-discussed, yet, I trust, not less interesting branch of medical polity—I allude to that department of forensic medicine comprised in the coroner's court.

The reformation of the medical profession has now been conducted under your auspices for the last fifteen years, and it is gratifying to add that, with few exceptions, its results have been characterised with complete and salutary success. Verily, British medical practitioners are under a deep debt of gratitude for your many ardent and laudable exertions on their behalf; and it is equally a source of surprise and regret to reflect, that no *general* demonstration of their acknowledgment has yet been made for your strenuous efforts in their cause. Let us not, however, impute to the medical fraternity any unworthy motive, in this their great dereliction of duty, but rather attribute it to the innumerable obstacles which intervene to prevent an assemblage upon any extraordinary occasion—this may appear to be the true explanation.*

Simultaneously, or nearly so, with the Corporation Reform Act, a measure which has been tolerably effective in renovating England's time-worn municipalities, appeared the Medical Witnesses' Act, a measure which was hailed by the members of our profession as a boon that was calculated (at the same time that it provided for their advantages) to confer no ordinary benefits on coroners' inquisitions, by instituting an uniform and searching investigation into the cause of sudden death, conformably with the rapid advance which has of late years been made in judicial medicine. Whilst, then, the former measure placed in the hands of common councilmen in each city and borough throughout England a discretionary elective power, which if properly employed, promised to carry out the provisions of the latter, medical men began to congratulate themselves that the time had at length arrived, when a thorough reformation was about to be effected in that important tribunal, the coroner's court.

Sensible that it would be culpable in the extreme, on the part of medical men, to permit an opportunity to pass that held out hopes of honourable preferment, and anxious that the attention of the common councilmen might be directed to the forthcoming election for coroner, I addressed (some few weeks prior to the election in 1835), a letter to the editor of one of our public prints, advocating the necessity of a medical coronership, and trusting that the choice of the Council might fall upon one of our own body. This communication created an argumentative correspondence between a respectable solicitor in this town and myself, which (judging from my opponent's subsequent silence) terminated, I flattered myself, not unfavourably to our cause. The day arrived, two medical candidates offered; one shortly withdrew, the other was not likely to succeed; the field remained uncontested, and a lawyer coroner was declared duly elected.

From that time to the present I have carefully watched the workings of the Medical Witnesses' Act in the Coroner's Court of this borough; and, so far as a rigid medical investigation into the cause of death, in doubtful cases, is concerned, I have no hesitation in saying that the objects of its framer have been diligently and systematically nullified.

* The honest discharge of a few public duties does not demand any such acknowledgment as has been suggested by our too kind correspondent.—ED. L.

Let us contemplate for a moment the sacredness of human life,—man's state of existence, viewed in connection with physical and moral agencies; its liability to be destroyed by causes frequently accidental, yet often of so questionable a nature as to render a rigid inquiry absolutely necessary, in order to arrive at a satisfactory explanation as to the true cause, and we shall immediately perceive that the tribunal before which such inquiries are instituted, is morally, nay, religiously, bound to possess information, alike remarkable for its comprehensiveness as its competency, that its fiat may stand in the opinion of the public beyond the pale of dispute.

Impressed with this conviction, the question necessarily follows, Are coroners generally in possession of that information which is essentially requisite to a proper fulfilment of their official functions? Or, in other words, are medical and legal acquirements combined in them? And if so, is that combination of knowledge brought to bear upon the investigations that come before a court?

The instances certainly are not numerous, in which medical and legal information is thus united in one individual, to render him adequately competent to a due discharge of the duties of an office so responsible as that of presiding power in a coroner's court.

Neither, supposing the judge to be well versed in judicial medicine, can many examples be adduced to prove that such information is made available in the inquisitions that come before a court. It would be altogether superfluous to multiply examples in order to support this twofold position. The daily occurrences in these courts are amply demonstrative that this assertion does not rest upon feeble or untenable ground.

Need we inquire into the cause of this? Or is our surprise excited by the circumstance, that our adversaries, being so exceedingly tenacious of their power, studiously resist not merely our claims to the coronership, but also to those of witnesses in these courts. Our opponents allege that such inquiries are "clearly *legal*, and can only be properly presided over by one who is conversant with the legal definitions of the various kinds of homicide, with the rules and nature of evidence, and with the powers with which the coroner is by law invested for enforcing the production of evidence,—keeping order in his court, and recording and returning the verdict in such technical form as that the prisoner may be tried thereon if necessary, and not acquitted in consequence of defects on the face of the inquest, as has too often been the case when medical coroners have been appointed."

True, to be thoroughly conversant with the legal department is to admit that a coroner in this respect is fully competent to fill his office. That medical coroners may have committed mistakes of a similar kind will not be denied; but for this reason, are we to deem it exclusively a legal inquiry, and are there no instances on record where, from defective medical information, legal coroners have *not* committed errors which have been equal to, if not worse than, the above?*

Again, it is held that the "duty of a coroner is to see that the inquiry is commenced, conducted, and concluded *according to law*, and if the deceased came (or from the evidence may be supposed to have come) by his death through the agency of another person, then to explain to the jury the oftentimes minute and nice distinctions between murder, manslaughter, justifiable homicide, death through misadventure, &c. &c., and to explain to the jury the bearings of the evidence upon the law as it is applicable to those various heads."

It is impossible not to be struck with the narrow-mindedness of the above quotations. Our opponents shrink from the admission that an acquaintance with medical jurisprudence is at all requisite in the conducting of the inquiry. *We*, however, unhesitatingly assert, that it is the duty of a coroner, who presides over an inquest, to see that the inquiry is commenced, conducted, and concluded, not merely according to *law*, but according to *justice*; and that it is indispensably requisite, in order to ascertain correctly whether a person has come unfairly by his death, that the presiding power be *medically*, as well as legally informed, and that in the strictest sense of the terms.

It cannot be denied, that in the majority of inquiries that come before a coroner's court, the jury are, or ought to be, *entirely* dependant for the validity of their verdict upon the testimony offered by a *medical*

* An individual was found dead with his throat cut; an inquiry having been instituted, the jury are directed, in consequence of the reception of incompetent medical testimony, by a legal coroner, to find a verdict of "suicide," which was recorded accordingly. Rumours, subsequently, tending to impugn the validity of the verdict, reach the ear of the mayor, who directs the *borough* coroner to hold another inquest, and take proper medical evidence on the occasion. Injuries are *now* discovered which deceased could not possibly have inflicted himself. This being coupled with other testimony, a revised verdict is found of "*wilful murder* against some person or persons unknown." Two men are subsequently executed on the most undeniable proofs for this offence, who had nearly been let loose on society to commit further murders, through the incompetency of a non-medical coroner.—LANCET, vol. i., 1836, p. 29.

witness, whether that evidence be negative or positive. Will it be believed that the members of the law manifest not the slightest reluctancy in the admission that a medical man ought "often" to be called as a "witness"? However, after admitting the necessity of the presiding power being well informed upon the legal bearings of the evidence, in order that it may impress the minds of the jurors with its applicability to the nice distinctions between murder, manslaughter, justifiable homicide, &c., it appears rather inconsistent to refuse the same power equal information upon the *medical* bearings, that by its influence upon the jurists they may be enabled to return a just verdict, by correctly contemplating the nature of the testimony afforded by a medical attendant.

It has already been alleged that the "Medical Witnesses' Act" is studiously rendered inoperative, and in confirmation of this assertion it is sufficient to state, that cases innumerable are continually occurring where medical evidence (founded upon accurate post-mortem examination) is alone capable of accounting for the immediate cause of death, and yet this testimony is overlooked. Take such an one as the following for an example:—An individual, who was on the previous day in the possession of good health, is discovered by another reclining over the back of a chair; he is examined and found to be quite dead; the rumour of the death spreads; the coroner is addressed; the jury is empanelled; the inquiry is made. From reasons which are sufficiently obvious, the *only evidence* upon which the decision should be founded is disregarded, and the very doubtfully accurate verdict of "Died by the visitation of God" is recorded! What testimony have we in such a case to prove that the deceased did *not* come unfairly by his death? Certainly none, and yet the members of the legal profession unequivocally assert that medical men ought frequently to be called as witnesses!

When we consider the rapid advances which forensic medicine has made within the last few years, the comprehensiveness of its range, and the indispensable information which it is capable of imparting to coroners in their inquiries, we must conclude that its study ought to be deemed of paramount importance. Every impartial individual who gives this subject the least attention, cannot fail to perceive that both sides of the question possess their advantages, and in order that such investigations may be effective, it is essentially requisite that both departments should be combined, and that *exclusive* medical or legal information is injurious, insomuch as it disqualifies the chief official on an inquest from a due discharge of his duties. Supposing coroners generally to be imperfectly educated, the question suggests itself, Who are the *most* eligible persons for the office, the members of the medical, or those of the legal professions? As the sole purpose for which such inquiries are instituted is to ascertain correctly whether a person have come unfairly by his death, and as this object is primarily, and in the majority of cases, solely attainable through a minute medical investigation of the body of the deceased, it follows that medical men are indisputably the chief claimants, and we can only regret that *their own apathy*, and the unfortunate bias of public opinion, should prevent them from filling these situations which are so zealously kept from them by their adversaries. I am, Sir,

Your obedient servant,

GEORGE LOWTHER.

Kingston-upon-Hull,
Jan. 1, 1839.

CREOSOTE IN DEAFNESS.

To the Editor of THE LANCET.

SIR:—I have observed the letters of Mr. Curtis, and J. T. and shall beg the indulgence of your pages for a few observations.

Dr. Manson, of Nottingham, published a work on the virtues of iodine, which cured, according to him, various diseases, particularly deafness: many thus afflicted, whose cases had long been considered incurable, were elevated with new hopes, only to be plunged into despair, in consequence of this *vaunted* remedy proving inefficacious.

A person, formerly an *itinerant aurist*, under one name, but who, by a little management, obtained his diploma under another name, and has returned to his *vagrant* life, attaching M.R.C.S. to his description in his advertisements, and stating that he has letters from Mr. Guthrie and Sir Astley Cooper, eulogising his abilities as a *surgeon aurist*, which letters, I am informed by those gentlemen, they *never wrote*, professes to perform wonders by *strychnia*. A man, however, who will publish deliberate falsehoods to impose on the public, is undeserving of credence. This new medicine has not obtained any reputation in such cases.

Next we had Dr. Turnbull, who wrote in favour of *veratria*, from which would be deduced, that this virulent preparation of hellebore was the panacea long sought to cure a variety of the ills flesh is heir to, among which deafness occupied a prominent place, but this, like the two former, excited hopes which were not to be realised; indeed, on the contrary, it inflicted considerable injury in some cases; among others the Duke of Wellington, who, without consulting Dr. Hume or myself, applied the veratria ointment around the ear, at the recommendation of some of his friends, which rendered him so very deaf during a considerable time that it was difficult to

make him hear any conversation. In another, a young lady, to whose ears this new remedy was applied, the effect was most unfortunate.

We have now Mr. Curtis advocating the use of *creosote*, and claiming the merit of introducing this article to public notice as an efficient agent for producing the secretion of cerumen in cases where it is defective. Before I enter into the question further, probably it will be more reasonable to discuss this portion of the subject:—In what manner does this excretion of cerumen into the auditory passage contribute to the perfection of the sense of hearing, or what are the uses of this substance? If Mr. Curtis cannot give a reasonable and satisfactory answer to this question I will endeavour to do so.

I have not noticed J. T., but think that a person who can write so to the point need not be ashamed to give his name, without which he cannot expect a reply from Mr. Curtis, or any other person. I am, Sir, your obedient servant,

W. WRIGHT.

Salisbury-square, Fleet-street,
Jan. 1, 1839.

CREOSOTE IN DEAFNESS.

REPLY TO MR. CURTIS.

To the Editor of THE LANCET.

SIR:—In a reply to Mr. Curtis, which appeared in your Journal of December the 8th, I endeavoured to prove that deafness was never produced by a deficiency in the secretion of the cerumen of the ear. In confirmation of my views I quoted the authority of Kramer; I brought forward cases which I offered to authenticate; and, lastly, I cited Mr. C.'s own cases, which latter, I anticipate, were alone sufficient to prove the inaccuracy of Mr. Curtis's views. To the above-mentioned remarks I perceive a reply from Mr. C. in your Journal of December 29th; he therein recapitulates his statements, and thereby thinks to convince the world of their truth. Authority, facts, and reason being in opposition to the views contained in these statements, had they emanated from a Herschell I should have disbelieved them; but when they have their source from one who (judging from the works having his name attached to them), displays the most profound ignorance of the subjects which he treats, then, indeed, am I loath to give them credence. To give some idea of Mr. Curtis's ignorance, I need only refer to *the sixth edition* of his work on the "Physiology and Pathology of the Ear," published in 1836. At page 78 it contains these words,—"all the small bones of the ear may be removed without causing deafness; but the stapes is the only one that *prevents the escape of sound from the internal ear;*" or I may recapitulate a sapient remark contained in his first letter, "that a person deaf from a disordered action of the chylopoietic viscera, may be cured by dropping creosote in the ear." When, immediately after the latter statement, Mr. Curtis calls upon his "professional brethren" to follow his example, I do, indeed, involuntarily exclaim, "O tempora! O mores!"

To prove that a deficiency of cerumen causes deafness, Mr. C. says, "that as Nature never made an organ in vain, nor gave it a peculiar function that was useless in the animal economy, it appears to follow as a necessary consequence that, either as effect or cause, the interruption of the function must be connected with some kind of disease." Is the knowing that an organ has a function equivalent to knowing what that function is? Because the cerumen must have some use, are we to conclude that that use is to assist in collecting or conducting vibrations of air? Do the hairs of the ear assist the hearing, or those of the nose assist the smell? No, they in both cases assist in keeping extraneous matter from the more delicate structures of the organ, and the mucus of the nose, as a means of arrest and expulsion of the more minute particles, may be compared to the cerumen of the ear. Supposing deafness to be caused by a deficiency of the latter, it would necessarily be attended by an accumulation of foreign matters in the meatus. In my former communication I called upon Mr. Curtis to authenticate his cases; his reason for not doing so is, forsooth, "because the verification of them would leave the question resting still upon his assertion, to be confirmed or neutralised by the experience of others, and no good result could follow." The only result that could and must follow, would be the eliciting of truth, and for this end I again call upon Mr. Curtis to authenticate his cases—upon him "whose first object is to do all the good in his power to suffering humanity, and whose second is to advance science in his peculiar branch to the utmost of his ability."* With your permission, Sir, I will suspend, for the present, any further remarks upon this subject, hoping that they will be rendered useless by a change in the conduct of Mr. Curtis. I am, Sir, your obedient servant,

J. T.

EXCITO-MOTORY SYSTEM.

DR. MARSHALL HALL AND MR. CARPENTER.

To the Editor of THE LANCET.

SIR:—As the party on whom the violence of the attack made by Dr. M. Hall on his critics, in his late speech at the Westminster Medical Society (LANCET Dec. 22), most

* Preface to the sixth edition of the Physiology and Pathology of the Ear, page 7.

heavily falls, I must beg your insertion of a defensive statement, which may set the question at issue between us more fairly before your readers than has been done by that gentleman. Having recently taken upon myself (in the preface to my "Principles of Physiology") the responsibility of the article in the "British and Foreign Medical Review" for April last, against which Dr. Hall's charges appeared to be principally directed, and being, as a young man (for I must plead guilty to *that* part of the accusation), desirous of acquiring a character with the professional public, which may suffer from his abuse, if not replied to,—it will not, I think, be wondered at that I should now seek to defend myself from the serious charge of having "sacrificed truth on the demon shrine of falsehood and malice."

It is not, Sir, my intention to reply to Dr. Hall in the style of language which he has employed towards myself and others, who venture to differ from him as to the merit of his researches. I have not, in the critique alluded to, used any language of this kind in reference to Dr. M. H., and I do not intend to begin now. All my statements have been *assertions of facts* or *expressions of opinion*. If I have wilfully perverted the first, it was for Dr. H. to prove it, *before* he stigmatised me at a public meeting, where I had no opportunity of defending myself, as guilty of *malignant falsehood*. If my opinions were incorrect, it was for him to have confuted them, before he denounced them as formed in a spirit of injurious depreciation. Dr. Marshall Hall has done neither the one nor the other. In all that he has published since the appearance of my critique, I can find not a single counter-statement,—not a single charge of a more definite character than that which he has last made. In replying to this, I hope to maintain the calmness of tone befitting a man of science and a gentleman. I have no intention of recriminating, my sole purpose is to defend myself. So long as the charges against me remain unsupported by the shadow of a proof, they recoil upon him who made them. And if, as I trust it will ultimately appear to all who impartially weigh our respective statements, my sole motive has been the establishment of TRUTH, whether in Dr. Hall's favour, or the contrary,—if it cannot be proved that I have wilfully perverted facts, and formed erroneous opinions, under the influence of malicious feeling,—it will be scarcely necessary for me to bring any heavier charges against him than those of which he will then have convicted himself. In the closing sentence of my critique I have thus expressed myself:—"In taking our leave of Dr. Hall, we would assure him and his partisans, that we have been actuated by no motive, in the examination into his claims which we have thought it our duty to make, save an honest desire to arrive at the truth, and to set their investigations upon their proper footing." Until, therefore, his two vague and sweeping charges of falsehood and malice have been verified by facts, I rely upon the justice of your readers, as a British jury, for being regarded as innocent of them. *I* DEFY *him to prove either the one or the other.*

I should have addressed you on this subject previously to the appearance of the last number of THE LANCET, had I not been desirous of satisfying myself, by private communication with Dr. Hall, that I was one of the parties involved in his charges on the occasion alluded to. His reply to my query is to this effect,—that, although he applied them at the moment to another rather than myself, he considers them as also including *any* anonymous critic who could write and print certain passages, to which he refers. It does not appear to me worth while to occupy a space in your pages, which may be much better employed, with the correspondence which has taken place between Dr. M. Hall and myself, during the last week; but Dr. H. has my full permission to publish it, if he considers it particularly creditable to himself, provided that he publish it entire.

I shall now, Sir, briefly set before your readers the appreciation of the merits of Dr. Hall's researches which I expressed in the article in question; and I would request those who are inclined to enter into the merits of the controversy, to read it for themselves, that they may be better prepared to estimate the meaning of the detached passages which must necessarily be adduced by Dr. Hall and myself. The two *general principles*, which it appears to me that Dr. H. has endeavoured to establish, are these:—

1. That *impressions* made upon particular parts of the body may produce reflex *motions* in the same or some other organ, through the medium of the *incident* nerve proceeding from the part stimulated to the spinal cord, the *efferent* nerve conveying the influence of the spinal cord to the part moved, and the portion of the cord included between the termination of one and the origin of the other (a single segment being sufficient when these points are identical); and that these actions may take place without the necessary excitement of *sensation*. This is the doctrine of the *reflex function* of the spinal cord. If my expression of it is incorrect I shall be glad for Dr. Hall to amend it; I have merely put it into my own language because the bearing of my critical statements will be thereby more readily perceived.

2. That these actions are performed by a system of nerves connected *only* with the spinal cord, distinct from those which ter-

minate in and originate from the brain; that the latter alone are concerned in the functions of sensation and *voluntary* motion; and that the brain, therefore, does *not* act through the true spinal cord, which is an independent organ. This is the doctrine of the *excito-motor system*.

To the first of these I have, as Dr. Hall's reviewer, given my full assent, and answered objections that have been raised against it, although I have questioned his originality in the enunciation of it. I have adduced the opinions of former physiologists in proof of my assertion that each of the propositions it involves had been previously sustained; but I have given to Dr. Hall full credit for having united them under one general principle, and for having worked out the details of its application; as the following passage will show:—"It appears to us that it is in the harmonious combination of these doctrines into a uniform system, and the application of it to the explanation of many phenomena which were formerly not regarded as explicable on such principles, that Dr. Hall's great merit consists, and not to anything peculiarly novel in the opinions themselves." It is to this general principle that I referred in my previously-expressed opinion that "the general results set forth by Flourens appear to us to contain nearly all that Dr. M. Hall can be said to have demonstrated;" an opinion of which I have fully set forth the grounds, so as to leave my readers to judge for themselves. I cannot be accused, therefore, of either falsehood or malice in expressing such an opinion; and it will be for Dr. Hall to show that I have wilfully perverted the facts on which I have grounded it, in order to substantiate his charge against me. This I call upon him, therefore, to do, since I think that it must be evident that, even if my opinion be untenable (and that it is not so I shall be able to produce some additional testimony when called upon to do so), it is not palpably deficient in probability.

The latter doctrine (that of the distinct excito-motor system, and all which follows from it), I have stated to be, in my opinion, not yet substantiated. This is a purely scientific question, to be decided only by evidence; and I conceive I am at perfect liberty to withhold my assent to it, until it is *demonstrated* to be *necessary* for the explanation of phenomena. I did not, in my critique, by any means pronounce upon it as an untenable hypothesis. I did my best to weigh the evidence on each side; and if the scale for the present turn in my mind against Dr. M. Hall on this point, it is only because I cannot see the necessity for the supposition. It is by no means unlikely that my own opinion may hereafter be brought to coincide with Dr. Hall's, if he can adduce stronger evidence in support of this doctrine; and, should this be the case, I shall be as ready as any one to admit its entire originality. At present I cannot be fairly accused of either falsehood or malice in refusing my assent to that which scarcely any physiologists of note have yet received as scientific truth, and against which such men as Professor Müller, Mr. Owen, and Dr. J. Reid (who, like myself, agree with Dr. Hall on the correctness of the *first* principle) have expressed a decided opinion. I adduce their authority (I beg Dr. Hall and your readers to understand) *not* as a reason for my belief, since I hope that my mind is sufficiently independent to form its own judgment upon the evidence, but as justifying myself from any charges affecting my impartiality as a critic. I trust, therefore, that Dr. Hall will see the propriety of confining his remarks on this part of my critique to its *scientific value*.

I regret to be compelled to occupy so large a space in your columns with a matter which may seem to concern *myself* alone. But I am sure that you will not refuse to me the opportunity of thus challenging Dr. Hall to substantiate the grave accusations he has made against me, and which have appeared from time to time in your Journal. If our controversy should continue it will be, of course, at your discretion to admit it, or not, into your pages. I shall only seek to justify myself from any fresh attacks which Dr. M. Hall may make upon me; and I will pledge myself to keep closely to the questions at issue. I am, Sir, your obedient servant,

WILLIAM B. CARPENTER.

Bristol, Dec. 31, 1838.

To the Editor of THE LANCET.

SIR:—I understand from a private note from Mr. Carpenter, that he has addressed a letter to you on the subject of a paragraph which occurs in some observations which I made at the Westminster Medical Society, about a fortnight ago, and I most willingly present your readers with the following remarks, the *last*, as I trust, that I shall be induced to make in reference to the shameful attacks which have recently been made upon me.

The first question doubtless is, Did the paragraph apply to Mr. Carpenter? And in reply to this question, I cannot say that it *did*, and I cannot say that it *did not*.

The truth is, that Mr. Carpenter has associated himself with about a dozen persons, who have recently undertaken to perform impossibilities, viz., to prove that my recent discoveries are *either* not new *or* not true; it does not signify which. In making use, then, of the words in question, I believe I meant *all* those individuals. Mr. Carpenter has, therefore, no right to claim more than one-twelfth of the force of those words as applied to himself,—to one-twelfth, I

think, you will consider him justly entitled.

One of this duodecemvirate writes, anonymously, of course, in an *evening paper* too! in reference to the experiments published in THE LANCET, for 1837-8, vol. i., p. 166:—

"Dr. Marshall Hall has within the last few days performed a series of experiments on a turtle, which had unfortunately fallen into his power, with the view of demonstrating to his class the nature of the nervous system. When experiments are made with the view of illustrating a disputed point, which has a direct bearing on any subject connected with the science of medicine, however much we deplore the necessity of such proceedings, we cannot condemn the operator; but surely those experiments ought not to be made merely to gratify idle curiosity."

It is true I rebutted this *malicious* insinuation in the following manner:—

"EXPERIMENTS UPON A HEADLESS TURTLE.

"Sir,—I have just seen, in your paper of this morning, a paragraph which, if the insinuation it contains were true, would be, and very justly, injurious to me. The writer forgot a material point,—*before the turtle came into my possession it was* HEADLESS *—had ceased to be a sentient being!*"

But then you know, Mr. Editor, that many read the accusation who never read the reply, so that the end of the writer is partly answered. I forget at this moment what Machiavellian writer says, "Accusez toujours," &c.

Another writer says,—"There is a copy of Prochaska's work in the library of the Medical and Chirurgical Society, and little as the volume was known to others, one member, at least, has shown that he duly appreciated its value, by the fact of his having repeatedly taken it out; that member, need we add, is,—Dr. Marshall Hall!"

It is true that the following note from the Secretary of that Society furnished the equally ready and sufficient reply, but, as before, *only* to those who chanced to read it:—

"Sir,—In reply to your inquiries, I beg leave to inform you, that you have taken from the library Prochaska's works on the following dates: Dec. 30, 1835, Jan. 30, 1837, Jan. 20, 1838;" that is, three years, and two years and a half *after* the date of my first publication.

A third writer emblazons his page with the following *italics*:—

"*Complete anticipation of Dr. Marshall Hall's doctrine of the reflex function by Prochaska.*"

A previous number of the same work contains *two* attacks upon my researches, one of these flatly contradicting the other,—the former stating that my observations were good enough, but to be found in Whytt and other authors; the latter, that they were new, but good for nothing, dangerous, and admitted by no one!

In a subsequent number of this same journal are no less than FOUR attacks upon me, attacks at once virulent, personal, false, and malicious. I have quoted the "heading" of one of these above. What do you think of it as a specimen? Now, Mr. Carpenter has written a review,—it forms one of the *four* articles above-mentioned. In it there is the following paragraph, amongst many others of the same kind:—

"The general results set forth by Flourens appear to us to contain nearly all that Dr. M. Hall can be said to have demonstrated." Now this I declare to be unfounded, for I have demonstrated, by experiment after experiment, and in a manner the most indisputable—

1. That there is an *incident motor* nerve.
2. That there is a *system* of such nerves.
3. That these nerves are the excitors in *all* the functions of ingestion and of egestion, of the orifices, and of the sphincters, in the animal economy, &c. &c.—See especially LANCET, for 1837-8, p. 131, § 5 and 6.

And I affirm, that there is *nothing* in the whole of the admirable work of M. Flourens, which *could* "appear," to any one of a judgment totally unbiassed by malice, *like* these "demonstrated" things; and that, therefore, the assertion is not founded either in truth or good feeling. Nay, there are many statements in M. Flourens' work totally at variance with this assertion. For example, again and again M. Flourens speaks of inspiration as having its *primum mobile* in the medulla oblongata, thus *excluding* the ideas of *incident excitor* nerves, the real *primum mobile* of inspiration.

I may add, that M. Flourens never once adverts to a *reflex action*, so that he is precisely *the* author whose "general results" do *not* contain *anything* that I have demonstrated.

I now appeal to you, Sir, and your readers, whether such things as I have briefly adverted to, are honourable to our literature, or to the profession at large, or just towards an individual who has laboured in that profession as I have done? And I would further ask, if I have felt and written indignantly, have I not also felt and written justly? I am, Sir, your obedient servant,

MARSHALL HALL.

14, Manchester-square,
Jan. 2, 1839.

THE LANCET.

London, Saturday, January 12th, 1839.

In our last article on the protective influence of vaccination we laid before our readers a very extensive series of documents, taken from the records of England, France, Germany, Italy, Sweden, and Denmark, which seemed to demonstrate that the protective power of the vaccine virus is considerably lessened by the lapse of time. The coincidence of results derived from the statistical observations which different writers have made in the principal kingdoms of Europe, and during a great number of epidemics, is so striking as to require little development or comment on our part. We find, however, that one important document had passed unnoticed amidst the variety of statistical records alluded to in our last Number. It is contained in an article "On Small-pox, as it appeared in London in 1836," by Dr. Gregory, Physician to the Small-Pox Hospital.*

The number of patients admitted into the hospital during that year was 329. The number of cases after vaccination amounted to 128. In the greater number the proofs of prior vaccination were as clear as the nature of the subject allowed, and by far the larger proportion of patients had been vaccinated in infancy. The relative ages of the 128 individuals, who were thus attacked by small-pox after vaccination, were as follows:—

Between 1 & 5 years (inclusive)	0
.... 6 & 10	2
.... 11 & 15	11
.... 16 & 20	40
.... 21 & 25	49
.... 26 & 30	20
.... 31 & 40	5
Beyond 50	1

From a consideration of this table, Dr. Gregory observes that it would be extremely difficult not to acknowledge "the "diminution or deterioration of vaccine security which lapse of years occasions." With this deduction we perfectly agree, and in order to render more clear the bearing of the facts upon which it is based, we shall here reinsert the tables of Möhl and Heim noticed in our last article. Instead of arranging them in periods of ten, we now divide them into periods of five years. The reader will thus be able to perceive, at a glance, how closely they resemble the preceding table of Dr. Gregory.

Of 653 cases of small-pox after vaccination, recorded during one epidemic by Möhl, we find,—

Between 1 & 5 years (inclusive)	14
.... 6 & 10	102
.... 11 & 15	173
.... 16 & 20	187
.... 21 & 25	156
.... 26 & 30	19
.... 30 & 33	2

Dr. Heim's table, though more extensive, is exactly similar to that of Möhl.

Between 1 & 5 years (inclusive)	40
.... 6 & 10	68
.... 11 & 15	186
.... 16 & 20	275
.... 21 & 25	239
.... 26 & 30	172
.... 31 & 35	75

It is impossible for any two statistical documents, which refer to an epidemic disorder, to bear a closer analogy to each other than do the above tables of Möhl and Heim. They indicate, in the clearest manner, a progressive frequency in the cases of modified small-pox, from infancy up to the period of manhood. They also point out an important fact, viz., that the maximum of cases occurs at the period which immediately follows puberty; thus rendering it probable that the modification of the system at that eventful epoch of life may exercise some influence on the protective power of the vaccine virus. The table of Dr. Gregory, it is true, indicates a maximum of cases between the ages of 21—25; but this discrepancy, as we immediately anticipated on perceiving it, depends on the comparatively limited number of cases which are recorded by the latter-named writer. If we

* British Annals of Med., Feb. 17, 1837.

combine together Dr. GREGORY's tables of 1823 and 1837, we obtain,—

Between 1 & 10 years	7
.... 11 & 15	24
.... 16 & 20	103
.... 21 & 25	95
.... 26 & 30	30
.... 31 & 40	5

Here, then, as in the tables of HEIM and MÖHL, the maximum of cases of modified small-pox occurs between the period of puberty and manhood; and the doubt which was at first raised by an examination of Dr. GREGORY's table of 1837, only served to confirm the law established by other writers.

Our desire to lay before our readers every circumstance which may throw any light on the important subject of vaccination, has induced us to dwell at considerable length on the preceding question, and to determine, as nearly as possible, how far the protective power of the vaccine virus may or may not be weakened by time. We shall now proceed to examine a point which is almost a necessary consequence of the fact that vaccine influence decays after a lapse of years. It is embraced in the following proposition, viz., "the gravity and "danger of modified small-pox are gene-"rally proportionate to the lapse of time "since vaccination." The professional reader will at once perceive that this proposition does not admit of the same strict demonstration by statistical evidence as did the relative frequence of modified small-pox according to age. The susceptibility of the constitution to disease, and the quality of the disease when received, are quite different things. The latter is subject to so many disturbing or modifying causes, especially in the case of an epidemic malady, that it becomes extremely difficult to determine how far the gravity and danger of the affection may depend on one particular circumstance rather than another. Having made this remark, we beg the attention of the reader to the following facts, which, without demonstrating, speak strongly in favour of the supposition, "that cases of modified "small-pox are not only more frequent, but "also more severe in proportion to the lapse "of time since vaccination." The quantitive and qualitive influences of the vaccine virus both decay towards the period of adolescence.

Amongst the vast number of facts which bear upon our present subject of consideration, we select but a few of the most striking. During the Copenhagen epidemic of 1824-27, described by MÖHL, and to which we have already so frequently alluded, amongst 257 cases of small-pox after vaccination, there were 16 which, for duration and severity of symptoms, bore the closest resemblance to variola vera. All these cases occurred in adults who had been vaccinated during infancy; three amongst them terminated fatally.

In the epidemic of 1828, the particulars of which are related by WENDT, 502 patients were received into the Small-Pox Hospital. Of these, 111 persons were affected with variola vera; 29 of the latter had been vaccinated during infancy; 4 of the vaccinated died. Amongst the 29 vaccinated persons thus attacked by true small-pox, there was only *one* child, of 4½ years of age; the rest were adults; not another child was affected by true small-pox during the period of this epidemic, and not a single case of the disease in any form occurred in persons who had been revaccinated.

The Swedish epidemic of 1835 was still more severe than that of 1828. The number of cases after vaccination amounted to 1013. The deaths amongst the latter were 47, but not a single fatal case was observed in individuals below eighteen years of age. Thus the severity of the disease, as indicated by its mortality, was great in proportion to the lapse of time since vaccination.

POMMER has given a minute and careful account of the epidemic which prevailed in Zuric during the year 1836. From 35 to 40 persons, whose ages varied between 8 and 34 years, were attacked by modified small-pox. None of the individuals attacked were cut off by the disease; but

POMMER remarked, *that the number of pustules, duration, and severity of the malady, were exactly in proportion to the extent of time which intervened between vaccination and the attack of small-pox.* Thus, persons who had been vaccinated eight or twelve years previously were affected with a slight eruption, which dried up and disappeared rapidly. Those vaccinated fifteen or twenty years before the appearance of the epidemic presented symptoms of a more serious nature, and the disease lasted for eight or ten days.

GAMBARINI assures us, that during the Milan epidemics of 1830, 1825, and those which preceded them, the intensity of the disease, and the abundance of pustules, were exactly proportionate to the lapse of time since vaccination.

The number of deaths from small-pox in Sweden, during the year 1834, amounted to 560, and amongst these 103 occurred in persons who had been vaccinated in their infancy; the whole of the latter were above fifteen years of age.

In one locality, during this epidemic, HEDLUND observed 69 cases of variola vera, and 91 of varioloid. Amongst the 69 persons attacked by true small-pox, 34 had been vaccinated with success. The whole of the latter were adults, and the date of vaccination was sixteen or eighteen years previous to the period of attack. Here, then, we find, that when small-pox attacks those recently vaccinated, it assumes the form of varioloid; but when the lapse of time between vaccination and the attack extends to sixteen or eighteen years, the malady frequently puts on its natural and unmodified appearance. This fact becomes more striking from the circumstance that individuals who had been vaccinated within ten years, escaped the disease altogether.

In the year 1831, a fresh epidemic of small-pox prevailed in Stockholm. Of 574 persons who were admitted into the hospital, 241 were attacked by true small-pox, 326 by varioloid, and 5 by varicella. *The disease affected adults who had been vaccinated during infancy with violence; it was mild and modified in proportion to the lapse of time since vaccination, and spared altogether infants and recently-vaccinated adults.*

After the evidence which is afforded by the preceding facts, it would seem superfluous to continue our present line of argument. We cannot, however, bring it to a close without alluding to a few analogous results which may be deduced from an examination of what occurred during the Württemburg epidemic.

HEIM, to whom we are indebted for such valuable information on every point connected with this inquiry, has given the ages of 186 persons who were attacked by true small-pox after vaccination.

From 0 to 5 years	6
5 — 10	9
10 — 15	31
15 — 20	46
20 — 25	39
25 — 30	30
30 — 35	15

As we recede from the period of vaccination the number of cases gradually increases up to manhood.

Dr. FRITZ observed 125 cases of varioloid in vaccinated persons. The moderate form occurred in individuals between 14 and 25 years of age. From 25 to 29 years, the malady was most severe, and approached nearest to true small-pox. Under 14 years of age it was very mild, and of short duration.

At Trossingen, Dr. RÖSCH remarked the following circumstances:—Seven children, below ten years of age, were attacked; five amongst them had the disease in an excessively mild form; none died. Between the ages of ten and twenty were attacked 29 persons; of these, one individual of eleven years, and another of nineteen died; two more were excessively ill; thirteen were were slightly affected, and in twelve the disease assumed a moderate degree of intensity. Between the ages of twenty and thirty there were twenty-two individuals; three were attacked in a mild, ten in a moderate, and nine in a severe degree; of the

latter, two died; a third death occurred in the person of a woman, who, both as a child and adult, had been vaccinated without success.

The conclusions of Dr. BODENMULLER, from the facts observed during his epidemic, are—

1. Children who have been vaccinated, up to eight years of age, though closely exposed to the contagion of small-pox, escape the disease.

2. When the vaccination is of longer date, individuals are affected by modified small-pox.

3. The revaccinated escape the disease in all its forms.

We should only fatigue the attention of our readers by a further enumeration of analogous facts. An example of what occurred in one family, amongst many, will suffice to bring this part of our subject to a close. A young man, 20 years of age, who had been vaccinated with effect when a child, was seized with true small-pox; four of his sisters, also bearing the marks of effective vaccination, whose respective ages were 8, 10, 12, and 14, escaped the disease; two other sisters, aged 21 and 23 years (both vaccinated in infancy) were seized by modified small-pox; the man's wife, who had been revaccinated, and an infant, remained free from the disease.

We now arrive at a division of our subject which, though extremely interesting, is but incidentally connected with the question of the decay of vaccine influence. The susceptibility of cow-pox is renewed after a certain lapse of time. Recent experiments, upon the most extensive scale, have demonstrated this fact in the clearest manner, but it does not logically follow that a renewed susceptibility of cow-pox indicates a renewed liability to small-pox. If, however, the doctrines which were laid down by JENNER be accepted without limitation, it would result that a secondary occurrence of vaccinia warrants us in concluding a liability to be affected by the contagion of small-pox; and in the present state of the science we do not perceive that any other doctrine can be feasibly supported. Let us, then, examine how far the susceptibility of secondary cow-pox is influenced by the lapse of time since the primary vaccination. Experience furnishes the following answer:—"Revaccination succeeds in proportion to "the time which has elapsed between it and "the original vaccination;" or, in other words, "the system refuses to acknowledge "the influence of cow-pox, as of small-pox, "for the first eight or ten years after vacci-"nation, but gradually becomes susceptible "of the contagion as the period is pro-"longed."

Revaccination has been performed to so small an extent in this country, that we are compelled to rely, exclusively, on the information which is furnished by other countries. Of these Germany assumes the chief rank, for the zeal and assiduity with which all questions connected with vaccination and revaccination have been therein studied. The following are a few of the observations made during the Württemberg epidemic.

Dr. SCHORDEN reports that revaccination seldom succeeded when practised five or six years after the first vaccination. The shorter the lapse of time the more imperfect was the secondary pock.

Dr. THEURER revaccinated 242 persons, in 124 of whom the cow-pock went through a *regular* course, exactly similar to that which appears in individuals who are successfully vaccinated for the first time. With respect to the ages of these individuals, Dr. THEURER remarks, that revaccination succeeded much more frequently in adult persons than in children, or the young, and that this occurred in a graduated proportion according to age; hence he concludes, "That the protective power of the vaccine "virus gradually decreases, while the sus-"ceptibility of small-pox returns in the same "proportion."

At Thalheim, Dr. RÖSCH practised 379 revaccinations. Up to the age of ten, the eruptions which were produced were extremely imperfect and modified. The great-

est susceptibility of secondary cow-pock was found to exist at the age of twenty; the least, at thirty years of age. Thus, up to ten years of age, the proportion of successful revaccinations was 11¾ per cent. From 11 to 15, it was 12 per cent.; from 16 to 20, 19 per cent.; from 21 to 25, 24¼ per cent.; from 26 to 30, 17 per cent.

Another physician, attached to one of the Württemburg districts, informs us that the shorter the period between the first and second vaccinations, the more irregular and uncertain were the results of the latter. Revaccination almost always failed when practised five or six years after the first introduction of the vaccine virus into the system. During one epidemic 106 persons were revaccinated: with full effect in 59 individuals; with doubtful result, in 38; without effect, in 9. The latter were the *youngest* amongst the 106 on whom the experiment was tried.

At Tubingen, after an epidemic attack of small-pox, several persons amongst the higher classes of society presented themselves for revaccination. Dr. LIPP observed that the operation generally succeeded in persons of twenty or thirty years of age; while in children, the introduction of the virus merely produced an irritation of the skin, which disappeared in a few days.

The few experiments which have been made in France seem to confirm fully the opinions of the German physicians whom we have just cited.

M. BAUDELOCQUE lately revaccinated 41 children at the Hôpital des Enfans with only a single success. M. DOUBLE, Member of the French Institute, revaccinated, not long ago, 16 persons whom he had vaccinated with success during their infancy. A complete cow-pock was developed in seven individuals. The virus taken from two of the latter was employed for vaccination, and produced full effect. M. RENAULDIN, also, has made some experiments on the subject of revaccination. In every case where the experiment was tried on persons below the age of twenty, it failed. On the contrary, revaccination succeeded in several instances where the lapse of time since the original vaccination exceeded twenty years.

In an epidemic which prevailed extensively throughout Provence, M. MAILLE practised numerous revaccinations. In children below the age of ten years he invariably failed to reproduce the cow-pock. When the period between the two vaccinations was more than fifteen years, he always succeeded.

The preceding facts are, we should conceive, sufficient to establish the truth of the proposition, "That the susceptibility of "secondary cow-pock is proportionate to "the lapse of time since the primary vac-"cination."

The "cui bono" of our researches, however, has not yet been touched upon. Should we revaccinate, or should we, as our predecessors have done, place implicit reliance on the protective powers of a single vaccination practised during infancy? The statistical facts which we have collected together in the present and two former numbers of this Journal, furnish a decisive answer to the latter question. We have, therefore, only to determine how far revaccination may be practised with success, and to what extent experience instructs us that revaccination will renew or revivify the decaying influence of the vaccine virus. These points shall be briefly examined in a concluding article.

UNIVERSITY COLLEGE AND HOSPITAL.

IT is currently reported, and we believe correctly, that Dr. COPLAND will finish the course of lectures on medicine which were commenced by Dr. ELLIOTSON in this College. Dr. CARSWELL has been appointed physician to the Hospital. Both these appointments are, of course, temporary, and will not in any way influence the elections to be made in the summer.

UNIVERSITY COLLEGE AND HOSPITAL.

MEETINGS OF STUDENTS TO DISCUSS THE RESIGNATION OF DR. ELLIOTSON.

In the last LANCET it was stated that an adjourned meeting of students would take place in University College on *Monday* to consider the subject of Dr. Elliotson's resignation; but as the Council were to meet on *Saturday*, the day of debate for the students was changed to the previous Friday, and on that day the meeting was accordingly held in the Anatomical Theatre, at 1 o'clock, immediately after Dr. Sharpey's lecture. Nearly 300 persons were present.

Mr. NEWCOME first rose and began to speak, amid cries of "We want a chairman." He said he had the authority of Dr. Elliotson for stating, that if the Council retracted their resolutions respecting the dismissal of Okey and the practice of mesmerism, he would return to his duties at the College and Hospital. The speaker, then, amid cries of "chair!" exhorted the students to come "like men to the charge," and support their teacher, but the confusion compelled him to sit down, and Mr. MACKENZIE, on one side, and Mr. HOFFMIESTER, on the other, were proposed to preside; the majority decided in favour of Mr. MACKENZIE, who accordingly took the office, though "with reluctance," as it was surrounded by difficulties. The subject of discussion was of great importance to the parties concerned. He requested that the speakers would be brief, and the voters uninfluenced by party feeling, as much weight would be attached to the result. Every speaker should have a fair hearing. (*Cheers.*)

Mr. CROFT proposed the first resolution. He called attention to the character of Dr. Elliotson, as a man of science, a gentleman, and a friend. In either case it would bear the strictest scrutiny. No one had done more towards simplifying the practice of medicine and establishing it on a rational basis. He had exerted more influence on its science and practice than any physician of the age. As a lecturer his success was equally great. When he commenced his duties in that College the number of pupils was little above ninety; now it amounted to nearly two hundred. To him were they chiefly indebted for the establishment of University College Hospital, where those rules of practice which he laid down in his lectures were illustrated, and their value confirmed. Every sentence of his lectures contained some sound practical information. There was no mixing of vague theories with his doctrines; in short, his lectures contained everything which the present state of our knowledge could supply. Explanations of any passages which required them were given in the kindest manner. If illness required his attendance on any of themselves, their friends, or their relations, who so benevolent as Dr. Elliotson? who more liberal or attentive? (*Cheers.*) It was not for the present meeting to discuss the causes which led to his resignation, or inquire into the correctness of peculiar views which he might entertain, those views having never formed a part of the lectures given in the College. These feelings he (Mr. C.) believed were entertained by all the students, and he should, therefore, propose to them the following resolution:—

"That the students of University College, duly appreciating the high professional acquirements of Dr. Elliotson, and the inestimable value of his services here, do most sincerely *regret the circumstances* which necessarily led to his resignation as Professor of the Principles and Practice of Medicine in the College, and as physician to the Hospital."—(*Cheers and hisses.*)

Mr. ROBINSON seconded the resolution. All his hearers must agree that the resignation of Dr. Elliotson would be one of the greatest calamities which could befal the institution, the interests of which were materially involved in it, for he was one of its strongest pillars, as he was also one of the brightest ornaments of the profession. Any one who had attended his lectures in the College, or had followed him for only a quarter of an hour through the wards of the Hospital, could not fail to derive instruction from his remarks and practice. Was this the man they were about to lose without one effort to restore him? At one time the voices of the University students were omnipotent (*laughter*); yes, omnipotent, as regarded the success of the objects which they sought to attain. In that case a professor (Pattison) was not satisfactory to them; they met, petitioned the Council unanimously, the lecturer resigned, and Dr. Quain was elected in his stead. There was no reason why the students on the present occasion should not succeed also. In coming forward he (Mr. R.) was not influenced by any private feeling, but solely by the interests of the establishment. (*Hear, hear.*) He had no intimacy with Dr. Elliotson; he admired him as an upright man, as a profound, reflecting, talented physician, who was gifted with great powers of observation, and facilities of communicating knowledge, and of laying down the principles of practice in the simplest manner. Were the students to be deprived of his services for merely prosecuting his inquiries in what he (Dr. E.) thought was a new science? If two or three pupils were lost by his proceedings in animal magnetism, what was that to the gain which would follow his re-appointment? He should not enter upon the subject of mesmerism, but he had yet to learn that it was "a mass of gross absurdity and deception." (*Cheers and hisses.*)

(The CHAIRMAN here requested that that subject, except in relation to the institution, might be avoided.—*Applause.*)

There was no proof (Mr. R. continued) that the inquiries into animal magnetism had injured the institution. On the contrary, the number of pupils had increased since Dr. Elliotson's appointment. As to the resignation, when Dr. Elliotson received the resolutions of the Council, the only step he could take was to resign (*Cries of "No, no," cheers, "Yes, yes."*) It was for the students now to pass the resolution of Mr. Croft *unanimously*. The Council would not turn a deaf ear to it *when so carried*. If Dr. Elliotson did not return, where would they find a successor? (*Cheers.*)

Mr. NEWCOME requested permission, amid cries of "*Yes, yes,*" and "*No, no,*" to make some remarks on mesmerism, but the Chairman thought that it would be better to leave it undiscussed.

Mr. DONALD inquired if this were a meeting simply of Dr. Elliotson's present class?

A GENTLEMAN said he considered it to be a meeting of Dr. Elliotson's friends, of his new and old pupils. He had attended as a friend of Dr. Elliotson and of the institution. For the welfare of both he had a regard, having been an old pupil of the College.

The CHAIRMAN said that it was a meeting of the students of the College generally.

Mr. GEORGE moved that the word "necessarily" should be omitted from the resolution. When the cause of the resignation came to be debated, it would then be found whether that resignation was *necessary* or not. Even if the motion was carried, the word *necessary* was not at all important, as its employment would not affect any resolution which might be proposed afterwards. He proposed this amendment in order that the students should know for what they really voted, and that the resolutions should be as distinct and clear as possible, not mixing one thing up with another.

The amendment being seconded,

A STUDENT said things were not mixed up together; the resolution merely called on the students to express their regret at Dr. Elliotson's resignation. The resolution was attempted to be made a mystery of for party purposes. (*Great confusion.*)

Another STUDENT proposed that the word "compelled" should be used instead of "led," in the original resolution. (*Laughter.*)

Mr. George's amendment having been negatived, the original motion was carried amid a storm of cheers and hisses.

A STUDENT said that Dr. Elliotson was *compelled* to resign.

Mr. CHAPMAN observed, that although animal magnetism was allowed by the Medical Committee to be employed as a remedial measure, it was well known that nine-tenths of the Committee entirely disapproved of the practice. From the manner in which the resolutions of the Council were conveyed to Dr. Elliotson he was not "necessarily compelled to resign." The Council did not wish to accept of Dr. Elliotson's resignation provided he abandoned the practice of mesmerism in the Hospital. (*Confusion.*)

A STUDENT.—We could not have Elliotson without the humbug of animal magnetism. (*Cries of "Turn him out;" "throw him over;" cheers, hisses.*) Did the meeting know what was done with Okey in the wards?

Mr. COOPER said the proceedings of the meeting were premature, the resignation of Dr. Elliotson not having been accepted by the Council, who had not met since it was sent in.

Mr. WARREN proposed the second resolution, and read an account, taken apparently from the work entitled "Elliotson's Physiology," of the Doctor's numerous discoveries and improvements in medicine. Dr. Elliotson, the paper stated, had established the stethoscope in this country, though when he first used it at St. Thomas's Hospital, he was laughed at for his pains. He had been the great advocate of prussic acid and quinine, in this country, and had effected their introduction into practice here, against the utmost opposition at first.—(*A voice,—"We know all this; it's quite stale to us;" uproar, and cries of "Turn him out."*)—He had been set down as a fool for giving large doses of carbonate of iron when smaller ones failed, in chorea. He enforced the use of sulphate of copper in chronic diarrhœa, and when he had ascertained that glanders occurred in the human subject, he was laughed at for his credulity. He had undergone a long series of attacks respecting his discoveries regarding creosote. Yet all the remedies which he thus advocated were afterwards brought into general use. (*Cheers.*) Would the students coolly put up with a successor to Dr. Elliotson, knowing the cause of his resignation? He did not resign willingly, but was compelled. Had he complied with the commands of the Council he might still have been lecturing, but only on the principle of disowning all faith in his own doctrines. His recall depended on the pupils themselves, as he (Mr. W.) believed that, at their request, the Council would withdraw their resolutions. (*Cheers and laughter.*) Dr. Elliotson would return if they were withdrawn, and the students, when their interests were so much concerned, should not be idle in the matter. Would any successor be so acceptable? (*"Yes!" "No!" cheers, hisses.*) They wanted him back; literally, they *wanted* him back—(*cheers and hisses*)—as a lecturer and a physician. He (Mr. W.) would move,—

"That the students of University College do most earnestly and strenuously, though respectfully, entreat the Council to adopt such measures as shall ensure the return of our esteemed Professor."

Mr. MOLINES seconded the motion. He believed that the students had a full right to express their opinions on a subject which interested them, although a gentleman in office had the day before gravely disapproved of the intention to hold this meeting. He thought a proper remonstrance to the Council would be attended to. (*Laughter.*) Independently of the inconvenient delay, the conclusion of the course by another lecturer would be most injurious to the students. He wished the remonstrance to the Council had been stronger. He called upon them to oppose those who would cry down University College School, and sympathise with their teacher, and as they valued his merits and services, though some seemed to forget past benefits, to restore him to the office from which he had been so unworthily expelled. (*Confusion and cheers.*)

Mr. JEAFFRESON moved an amendment, that the meeting did not impute blame to the Council in the steps they had taken with respect to Dr. Elliotson. He was perfectly ready to admit that Dr. Elliotson was a man of high talent and abilities, and had rendered great service to medicine and to the College, and that it would be inconvenient for the class to attend the remaining half under another lecturer; but no one had proved that the blame of the resignation rested with the Council. On the contrary, it was well known that the resignation arose out of Dr. Elliotson's exhibitions in the so-called *science* of mesmerism in the Hospital, to which most of his colleagues had strong objections. (*Cries of "Order."*) The reputation of the Hospital had been injured by these exhibitions. (*Cries of "No!" and "Yes!"*) He had no wish to discuss the truth or falsehood of animal magnetism, but no one would doubt that its use had affected the reputation of the Hospital. It was known that a mesmerised girl was taken to the bedside of patients to prophecy the result of the cases. (Here the opposition uproar became so great that the proceedings were altogether stopped.) On silence being obtained,

A STUDENT complained that Mr. Jeaffreson had been allowed to speak on animal magnetism, while other gentlemen had been prevented.

The CHAIRMAN said, that it should be discussed only so far as its practice had influenced the determination of the Council.

Mr. JEAFFRESON would not exceed that limit. Okey, he was about to say, had been to the bedside of a patient to tell whether he would die, Now, whether animal magnetism was true or not, such a proceeding was calculated to injure the Hospital. (*Uproar from Dr. E.'s friends.*) It was the legitimate object of the Council to protect the Hospital from such acts. Dr. Elliotson had received repeated remonstrances on the subject from his colleagues. When the House-Committee took up the subject, Dr. Elliotson did not deny that he had taken the girl into the sick wards. He raised no objection to the statement of the nurse before the Committee to this effect, and the Council judged, from the evidence before them, that the institution was seriously injured by these proceedings, and they were perfectly justified in requesting Dr. Elliotson to discontinue them. Indeed, it would have been a gross dereliction of their duty had they failed to do so. He had himself known instances of pupils refusing to enter to the Hospital on account of the mesmerism. (*Cheers and hisses.*) As the public mind was at present constituted regarding animal magnetism, its practice must be injurious to the institution. (*A voice,—"Look to the number of students this year."*) Looking at the facts, he did not see how a more courteous mode of proceeding towards Dr. Elliotson could have been adopted. The Council passed two resolutions. One, that Okey be immediately discharged; the other, to prevent further mesmerism in the Hospital. The House-Committee, acting upon these resolutions of the Council, "respectfully requested Dr. Elliotson to discharge Okey." Could a more gentlemanly step be taken? It would be a great injustice to the Council to imply, by a vote that day, that they ought to make an apology to Dr. Elliotson for what they had done. He would render to Dr. Elliotson all that justice required, but he would not eulogise him at the expense of casting unmerited obloquy on the Council.

Mr. GEORGE rose (amid cheers and hisses) to state a few facts to the meeting. He advised the opposing parties to remember that facts were better arguments than hisses. They might meet them with hisses, but the facts would still remain the same. In an anonymous letter which was printed for circulation among the students, he had been charged with adjourning the meeting of Thursday until Monday, because the Council would hold a meeting on Saturday, before the students could be heard. The libeller had called this a *ruse* of his. He should not notice this contemptible letter, but that its contents might affect the observations which he intended to make. He trusted, however, he was too well known to the students to be considered guilty of such a proceeding. The statement was entirely false, and perfectly cowardly, and its author was a scandalous libeller. (*Great cheering.*) He thought as highly of Dr. Elliotson as any speaker, and he had been much indebted to him for medical knowledge, and the institution owed him much, but he (Mr. G.) now came forward for the welfare of the institution itself. At the meeting on Thursday the parties chiefly interested wished to press on the subject with what he considered indecent haste. To prevent this he had

moved an adjournment, to give time to consider well the steps which the students were about to take. They were now assembled in consequence of a letter, in Dr. Elliotson's handwriting, which had been freely circulated among the pupils, stating, that as the Council had ordered the instant dismissal of a hospital patient, without making the slightest communication to him, and had, moreover, ordered him to discontinue mesmerism in the cure of disease in the Hospital, he had no alternative but to resign. (*Cheers and hisses.*) Now, what were the facts? This was not the first application to Dr. Elliotson to discontinue mesmerism. Members of the Council and the Committee had repeatedly pointed out to him the injury which it was doing to the school. Dr. Elliotson had stated in his letter that Okey was ordered to be instantly discharged without any communication being made to him. Now, the official documents of the Hospital, which, as an officer of that institution, he had seen, showed, that at a meeting of the Committee on the 13th of December, which Dr. Elliotson was invited to attend, fixing his own time, various statements were made regarding Elizabeth Okey. One rule of the Hospital was, that if any patient remained under treatment for more than two months, if no report of the case were sent by the medical officer in attendance, that patient should be discharged. A member of the Committee, knowing that Okey had been two months in the Hospital, brought the subject forward, and Dr. Elliotson was asked why he retained her. He replied, that he had intended to give notice of her retention, but the Committee had forestalled him. The Committee having afterwards heard Dr. Elliotson's reasons for detaining this patient, and also other evidence on the subject, referred the matter to the Council, who met on the Saturday week following, and who, on taking the whole matter into consideration, resolved,—"That the House-Committee do forthwith discharge Okey." But did the House-Committee immediately do this? It would be thought, from Dr. Elliotson's letter, that they had ordered their officer to eject her at once. No such thing, the Committee sent a polite message to Dr. Elliotson, "respectfully requesting him to discharge Okey." The question was one of right, and was acted on as one of courtesy. Her stay was injurious to the Hospital. So much for the right. As to the manner, could anything have been more courteous? And so courteous were the proceedings, that even yet she was not discharged; at that very moment—while he was speaking—she was in the Hospital. Nay, more, she had been magnetised more than once *since* the resignation (looking towards Mr. Wood, who sat opposite),—not a very decent proceeding, he thought, all things considered. (*Cheers.*)

But the letter of Dr. Elliotson also said that he had been forbidden to cure patients by mesmerism; yet what was the resolution? Why, "That the Hospital Committee do take steps to prevent the further practice of animal magnetism in the Hospital." Had mesmerism been confined to attempts to *cure patients*, the Council would not have interfered, but the manner of its employment must have had a demoralising influence in the institution, which, moreover, became, in Okey's case, a board-and-lodging-house. Was the taking of Okey to the houses of Mr. Wakley, Dr. Roget, and others, to be exhibited to different audiences, an employment of mesmerism as a means of *cure*? Was the Hospital to be made a board-and-lodging-house for mesmeric girls, for the convenience of taking them from one house to another, to procure worshippers of the new science? (*Uproar.*) Prophecy might be a good thing and true, but he would ask the students of a grave and important science what they thought of the senior physician of a London Hospital resorting to mesmeric prophecies as a means of assisting his diagnosis? (*Cheers and hisses.*) The science must, indeed, be at a low ebb when such practices were resorted to. Granting, even, that this girl *did* possess prophetic power, was it decent, proper, or moral, for a girl in a state of "mesmeric delirium" to be taken to the bedside of a dying man to predict the result of his case? When so taken she said that the phantom which sat on his bed was so horrible that she was afraid to look at it! Was this the curing of patients by mesmerism? Should the Governors pass over this kind of proceeding in silence? (*Uproar.*) Imagine the effect on an enfeebled patient. A girl, whom his physician supposes to be gifted with the power of prophecy, the effect being heightened by her delirious manner, approaches his bedside, and prognosticates *that he will die!* It was bad enough for the physician to place confidence in such predictions, but what would be the effect on the patient? Was it to be wondered at that this patient did really die? (*Uproar, and cries of "Prove it."*) It had been proved. The facts were given in evidence before the House-Committee, at which Dr. Elliotson was present, and Dr. Elliotson did not deny them. The nurse, it is true, said, that she did not *know* whether the patient on whom the prediction was passed heard it, but that the patient in the next bed certainly did. (*Renewed uproar, and cries of "Hear, hear."*)

Mr. TAYLOR rose to say that Dr. Elliotson was not present to be confronted with the witnesses. He was not there as an accused party, but was merely called in as a witness, like the other witnesses, to give evidence as to facts.

Mr. GEORGE accepted the correction. All that he desired was the truth, in order that

the students might judge for themselves. (*Bravo.*) Mesmerism might be true or false. (*Cries of "Question."*) He was not now going to deviate from the question, but this he would say, that carried on as mesmerism had been, in defiance of the whole profession, it must be injurious to the institution. (*Uproar.*) Mesmerism was so disgraceful to the character of a public institution—(*cheers and hisses*)—so derogatory to the dignity of a physician—(*cheers and hisses*)—so opposite to everything that should be expected from Dr. Elliotson—(*cheers and hisses*)—that he blamed the Council for not having stopped it before. (*Cheering and uproar.*) To represent to young and ingenuous minds that an individual had been persecuted, was enough to make their hearts rebound in his favour, but before the students on this occasion gave their votes, he trusted that they would understand the case. If they passed a resolution to the effect that the Council had done wrong, and they afterwards repented of the step, they would give the Council a reason for not listening to them on another occasion. As to the facts which he had laid before the meeting, he called on any one present to gainsay them; he paused for a reply. (*A silence of half a minute was observed, without interruption, but no reply was made. Great cheering then ensued, without the generally attendant uproar.*) Would, then, the students wish for a repetition of such facts as he had recounted? Did they wish wild "prophecy" to take the place of scientific diagnosis? To have the patients again disgracefully and disgustingly alarmed in the Hospital? (*Cheering and hisses.*) If not, how could they demand the restoration of Dr. Elliotson? Assuming that Dr. Elliotson had been ill-treated by the Council, why did he retaliate upon *the students?* Why stop in the middle of a course, when his services were particularly valuable? An inferior successor had been talked of. Who made the vacancy for the "inferior" man, to the injury of the students? Their Professor need not have resigned; if forbidden to mesmerise in the *Hospital*, he was not forbidden to lecture in the *College*. (*Great cheering.*) And as to the question, where they were to look for a fit successor, was there no balm but in Gilead? Were there no able professors in other schools? Eminent as was Dr. Elliotson, they would be better off with a man who, even though a little inferior, would act a little more respectably. (*Confusion.*) He spoke of Dr. Elliotson only as a physician in a public hospital. The Council were treated by one part of the meeting as though they were opposed to the welfare of the institution; why, they could have no interest in injuring their own establishment. The Council could only prosper as the College prospered. The interests of the students were the interests of the Council. It was the Council that elected Dr. Elliotson,—all their professors—and they were now about further to promote the interests of the institution. Though, in conclusion, he should not object to second Mr. Jeaffreson's motion, he would rather substitute the following. (Mr. Jeaffreson assented.) Mr. George's amendment was to the effect,—

"That an address be presented to Dr. Elliotson, thanking him for his past services, and expressing the regret of the students that they were deprived of them for the future."

Mr. Wood had not intended to take any part in the proceedings, because his interference would by some be attributed to improper motives, but as the proceedings of the House-Committee had been brought forward in such an extraordinary manner before a public meeting, he felt bound to communicate some facts that he was aware of relating thereto. He held in his hand the letters of two professors of the institution, stating that they believed in mesmerism, and acknowledging Dr. Elliotson's right to proceed with the investigation in the Hospital. ("*Names, names.*") There was no occasion to mention names. (*Cries of "Oh! oh!" and an objection from the Chairman to the speaker pursuing the subject of mesmerism.*) It had been intimated that Dr. Elliotson's letter was circulated amongst the students for an improper purpose. But the only object of its circulation was to give information to the students, as it did at the end, that a due portion of their fees would be returned to them. True it was, that repeated applications had been made to Dr. Elliotson to do away with the "humbugging" proceedings, as some called them;—(*hear, hear*)—but he had in his possession a letter from the Medical Committee recognising his right to apply mesmerism as a remedial agent. (*A voice,—"As a remedial agent, mind you."*) At the time that this resolution was passed public feeling had not been awakened against animal magnetism. When Dr. Elliotson saw that the feeling of his colleagues was against the public exhibitions, he consented to discontinue them, but still acted on the permission of the Committee in employing it as a remedial agent. As to the rule that no patient should be in the Hospital more than two months, there was now a patient under Dr. Thomson's care, who had been in the wards above a year, without objection from the Committee. Another patient of that physician had been under treatment for six months. Yet, in the case of Elizabeth Okey, the Committee had demanded to know the cause of her retention before she had been two months in the house from the time of her readmission. (*Hear, hear.*) She had been in the Hospital previously for nearly a year for epilepsy, and was discharged as cured. Her last admission was for ischuria. After the

investigation before the House-Committee it was arranged between a member of that Committee, a member of the Council, and Dr. Elliotson, that Okey should remain in the Hospital until February next. Yet two days after this arrangement the Council met and passed those resolutions which compelled Dr. Elliotson to resign. He said *compelled*, for surely he *was* compelled to do so. Dr Elliotson had been accused of indecency and immorality in the performance of mesmeric experiments.

Mr. GEORGE explained that he applied the epithets, indecent and demoralising, to the *experiments*, performed by whomsoever they might be, not to Dr. Elliotson personally.

Mr. WOOD.—The man who countenanced demoralising things was himself immoral. (*Hear, hear.*) It was said that the Hospital had been used as a board-and-lodging house. What were the facts? The Committee last summer requested Dr. Elliotson to remove the patients under experiment to a house in the neighbourhood,—to anywhere out of the Hospital,—to a public-house if he pleased—(*ironical cheers*)—if he considered the experiments necessary to prove the truth of mesmerism. He did take the patients away; he took them to the house of Dr. Roget, who was at that time, if not now, a member of a Committee which was formed to inquire into the subject. The girls were also taken to Dr. Elliotson's house, to be experimented on before a number of eminent legal gentlemen, who were almost all, if not all, satisfied with the reality of the phenomena. They had also been taken to the house of Mr. Wakley. He adverted to that fact with great reluctance. (*Laughter, and cries of "You'd better leave it alone."*) He was fully prepared to explain all that took place there. A gentleman was present at the meeting to-day who had before watched the experiments, day after day, and who had *felt* fully satisfied that there was no trickery practised, and who had frequently requested Dr. Elliotson to take the girls to Mr. Wakley's house, who, he said, would be satisfied if only two experiments were successful. He did not know what was that gentleman's opinion of mesmerism now. The proceedings in the Hospital were said to be *disgraceful*. He should be ashamed to own any fellow-student who would apply such a term to Dr. Elliotson. (*Confusion.*)

Mr. RAYNE requested to ask Mr. Wood if he had seen the reports of animal magnetism in THE LANCET in June last, and if he approved of the language employed by one of the mesmerised girls?

Mr. WOOD would ask, in reply, if any physician was accountable for the ravings of a delirious patient?

Mr. GEORGE.—Yes, if he produced her delirium.

Mr. WOOD was sure that Dr. Elliotson pursued his investigations merely for the sake of science and the cure of disease. He had no personal interest to serve in the matter; he could only be a loser by the prosecution of his inquiries. Public prejudice and party feeling were opposed to him. Instances, however, were constantly occurring, in all parts of the country, of cures effected by mesmerism; and how was it proved that the institution had been injured by mesmerism? Was it by the fact that Dr. Elliotson's class was this year larger than ever. (*Cheers.*) He knew of no falling off in the number of entries at the *Hospital*. How, then, dared the Council to interfere with Dr. Elliotson's practice, and prohibit him from doing this or that in the Hospital? (*Oh! Oh!*) It was proposed, in the amendment of Mr. George, to send an address to Dr. Elliotson, but he was sure that Dr. Elliotson would not receive it. He would consider such an address as an insult; it would appear, if that amendment were carried, that the Council had done right in discharging him, as incompetent to fulfil his duties. This was the only construction that could be put upon such a proceeding. As to the resignation, when Dr. Elliotson said that he would never enter either the College or the Hospital again, of course that was said conditionally. If the resolutions of the Council were withdrawn, Dr. Elliotson would return and do his duty. (*Cheers and uproar.*)

Mr. PENNY said that the *Council* looked upon the affair as one of money,—the *students*, as one of principle. If Dr. Elliotson had not already been insulted enough, what limits would gentlemen place to insolence?

Mr. GEORGE rose to explain. It had been said that an arrangement was made between Dr. Elliotson and a section of the Committee, on the subject of retaining Okey in the Hospital until February. Now the Committee of the Hospital was under the control of the Council, and answerable to them for its proceedings. The Council had, therefore, a perfect right to overrule any measure of the Committee. (*Hear, hear.*) He denied having attacked Dr. Elliotson personally; he only wished him to restrict himself to the legitimate paths of science.

Mr. JEAFFRESON made the same avowal.

Mr. COLES inquired whether it was not better to put up with Dr. Elliotson's foibles than have an inferior man. The College would lose less by taking him back and allowing him to mesmerise. He saw nothing like indecent haste in the meeting on Thursday, though the gentleman who was so anxious to announce that he held office in the Hospital wished to postpone it to Monday, when it would be too late to make any effort to save Dr. Elliotson, and they would have to be contented with sending a mere letter of condolence to him. He thought

that Dr. Elliotson had not had proper credit given to him for the latter part of his letter, respecting returning part of their fees to the students, because with that money the students could go to any school they chose for the rest of the course.

Mr. COOPER suggested a resolution which would not impute blame either to Dr. Elliotson or the Council, between whom, he trusted, the matter would be made up.

Mr. JEAFFRESON insisted that blame was not attached to any party in the amendment.

A STUDENT said that the original motion was carefully drawn up on purpose that blame might not be imputed to any one.

Mr. DONALD proposed, and Mr. MILLETT seconded, an amendment to the effect, that the students regretted the resignation of Dr. Elliotson, and trusted that the differences between him and the Council would be adjusted, and that their esteemed Professor would be speedily restored to them.

The cry now arose to put the question to the vote. The Babel of tongues was most confusing, and the inadequacy of the Chairman to preside over so stormy a meeting, and the total ignorance which everywhere prevailed as to the proper mode of dividing on motions and amendments, became prominently manifest. There were before the chair three motions, that of Dr. Elliotson's friends, that of Mr. George, and that of Mr. Cooper.

The first division was taken on Mr. Cooper's amendment, which had but very few supporters, perhaps a dozen.

Then Dr. Elliotson's resolution was put, and declared by the Chairman to be *lost* by a small majority, perhaps five or six, amidst loud cheers from its opponents, as it was said that the chief agitators had expected a majority of three or four to one in favour of the Doctor.

But the original resolutionists were so much dissatisfied with this result, that Mr. George yielded, after a poor and inadequate remonstrance, to their call, on recovering from their surprise, to put *the original resolution also!* This, after the lapse of a few minutes, was done, when the Chairman declared the numbers to be equal, whereupon Mr. George's party called upon him to give the casting vote, having first protested that, on the decision being announced in their favour, some of their voters had quitted the theatre, not suspecting the irregularity which would be permitted to ensue. Before, however, this demand could, amid the clamour of each side, fall with sufficient weight upon the mind of the Chairman, a claim from other parties was enforced to have an entire renewal of the dividing process. Hereupon the amendment of Mr. Cooper was again put and negatived, and then a division was taken on Dr. Elliotson's resolution, which this time—apparently verifying the allegation that some of the supporters of the amendment had left the place—was carried by a majority of two or three. The Chairman did not announce the figures, but on both occasions the numbers appeared to approximate on each side to 90; about a third of those who were present not voting at all.

In this perplexing state of affairs Mr. GEORGE moved that the question be put to the ballot, between the hours of 1 and 3 o'clock, in the beadle's room, on the next day, Saturday, Mr. Mackenzie (the Chairman) and Mr. Taylor, being appointed scrutineers. The motion was at first vehemently opposed by the other side, but subsequently put and carried on a division. The next question was, Who should vote?

Mr. GEORGE moved that those students who were attending the College at present, or had perpetual tickets to the Hospital, should be allowed to vote. This was fair for all parties concerned, and then there would not be voters coming north, south, east, and west to make a majority of persons who had no direct interest in the matter.

Mr. WOOD protested against the ballot altogether, but without avail, and the qualifications having been settled, the meeting separated, with a vociferation of "Three cheers for Dr. Elliotson and mesmerism!" and "A little one in for Okey!" but from which side the proposition came we cannot say, as it would have been equally *apropos* from either.

January 5th.—On Saturday, before one o'clock, a great number of students had collected in the beadle's room, but there was still a disagreement respecting those who had a right to vote. Mr. Taylor, also, sent to decline the office of scrutator, which added to the confusion. Mr. Wood vehemently protested against the place of ballot, and moved that the box be removed to the theatre, where, at last, the parties adjourned, Mr. Mouat being elected to the chair. Mr. Taylor now withdrew his resignation, and the original proposition of Mr. George, as to the qualification of voters, was adopted. Great exertions were then made by the late Professor's party to bring up voters. They beset every avenue leading to the theatre, like canvassers at the old elections. The scene in the theatre was exceedingly animated and amusing, and as the time approached for closing the ballot (extended to half-past 3) the anxiety depicted on the countenances of the actors and spectators was extreme. As the clock was on the point of denoting the half-hour,

Mr. WOOD arose, amid great din, and said that the charge brought against Dr. Elliotson, that he had taken Okey into the wards for the purpose of hearing her "prophecy," as stated yesterday was false, utterly false!

Mr. GEORGE appeared on his legs, and waited for several minutes to get a hearing from the other party, by aid of the

Chairman, who said, that although the time of the ballot had expired, yet it would be unfair if he did not allow Mr. George to answer the charge of falsehood which was brought against a statement that he had made. Mr. George then said that the words he had used regarding Okey were to the effect that she had been taken into another ward for the purpose of passing an opinion, or of prophecying, as to the result of a case. (*Mr. Wood,*—"*You said prophecy.*") He would thank Mr. Wood to interpret his own language, and not to be a better judge of what he (Mr. G.) said, than he was himself. What he had said at the meeting on Friday was strictly and entirely true.*

The ballot was now declared, when there appeared,

For the resolution	124
For the amendment.....	113
Majority for Dr. Elliotson and mesmerism	11

Both parties considered the result to be a victory, and united in cheers. When order was obtained,

Mr. PETERKIN proposed, and Mr. COLE seconded, that the Chairman be requested to forward the resolutions to the Council at their next sitting.

Mr. GEORGE wished to move an amendment, which was opposed by the 124. The Chairman decided that no amendment could, consistently, be put.

Mr. GEORGE disputed the decision. On a specific resolution he had a right to move an amendment. His reason for so doing, was this, that as the Council might wish to act on the feelings of the majority, it was right that they should know what the extent of that majority was, as also the nature of the amendment for which the 113 had voted. For this reason he wished to move that information on both points should be sent to the Council.

Mr. WOOD protested against this course being adopted.

Mr. COLE thought it would be absurd to forward Mr. George's amendment. If they did so, of what use was the meeting?

Mr. DONALD contended that all further opposition to the resolution must be factious.

Mr. GEORGE said there was no occasion to deny that accusation.

Much cheering attended the observations of either party, until the assembly was roused to a pitch of the greatest excitement, amid which a friend of Dr. Elliotson kept calling out, "It's just four o'clock, we shall be too late," alluding to the meeting of the Council at 4 o'clock, and expressive of his anxiety that the result of the ballot should be laid before the Council previous to its entering upon the business of the College. Mr. George's amendment was then put aside by a negative, and the resolution which was rejected at the meeting on Friday, and adopted on Saturday, was conveyed to the Council, but that body had just disposed of the resignation of Dr. Elliotson, by accepting it.

It should be added, that however eager in their demonstrations of disapproval were the divided parties at these meetings, the whole proceedings were conducted with excellent temper. It should also be stated, that the feelings of the junior students were very naturally enlisted in favour of a Professor from whom they had personally heard nothing respecting the impostor Okey, and mesmerism, in his lectures in the College. The value of these have not yet been diminished by placing them on the new basis. Since the commencement of the October session, no part of his addresses have been devoted to animal magnetism, and the new students could, therefore, only become personally acquainted with its *practice* by seeing clinical clerks manipulating in the wards on epileptic patients, or with its *deceptions*, by accidentally stumbling upon Okey during the private experiments. Dr. Elliotson may thus be regarded by them as an unexceptionable physician and teacher, and a persecuted man.

* Whatever meaning may be given to the word *prophecy*, it appears that Dr. Elliotson wrote down the remarks of Okey on cases in which she professed to see either "Great" or "Little Jackey" on the bed, sealed them up, and gave them to Mr. Taylor to preserve against time should verify or disprove them.

ROYAL COLLEGE OF PHYSICIANS OF LONDON.

(*Recently issued by the College.*)

THE College of Physicians having for some years past found it necessary, from time to time, to make alterations in the terms on which it would admit candidates to examination, and license them to practise as physicians, has reason to believe that neither the character nor object of those alterations, nor even the extent of the powers with which it is invested, has been fully and properly understood.

The College, therefore, considers it right, at this time, to make public a statement of the means which it possesses, within itself, of conferring the rank and privileges of physicians on all those who, having had the advantage of a liberal education, both general and professional, can prove their qualifications by producing proper testimonials, and submitting to adequate examinations.

Regulations regarding Certificates and Testimonials.

Every candidate for a diploma in medicine, upon presenting himself for examination, shall produce satisfactory evidence,—

1. Of unimpeached moral character;
2. Of having completed the twenty-sixth year of his age; and
3. Of having devoted himself, for five years, at least, to the study of medicine.

The course of study thus ordered by the College comprises,—

Anatomy and physiology; the theory and practice of physic; forensic medicine; materia medica and botany; and the principles of midwifery and surgery.

With regard to practical medicine, the College considers it essential that each candidate shall have diligently attended, for three entire years, the physicians' practice of some general hospital in Great Britain or Ireland, containing at least one hundred beds, and having a regular establishment of physicians as well as surgeons.

Candidates who have been educated abroad will be required to show that, in addition to the full course of study already specified, they have diligently attended the physicians' practice in some general hospital in this country for at least twelve months.

Candidates who have already been engaged in practice, and have attained the age of forty years, but have not passed through the complete course of study above described, may be admitted to examination upon presenting to the Censors' Board such testimonials of character, general and professional, as shall be satisfactory to the College.

The first examination is in anatomy and physiology, and is understood to comprise a knowledge of such propositions in any of the physical sciences as have reference to the structure and functions of the human body.

The second examination includes all that relates to the causes and symptoms of diseases, and whatever portions of the collateral sciences may appear to belong to these subjects.

The third examination relates to the treatment of diseases, including a scientific knowledge of all the means used for that purpose.

The three examinations are held at separate meetings of the Censors' Board. The *viva voce* part of each is carried on in Latin, except when the Board deems it expedient to put questions in English, and permits answers to be returned in the same language.

The College is desirous that all those who receive its diploma should have had such a previous education as would imply a competent knowledge of Greek, but it does not consider this indispensable, if the other qualifications of the candidate prove satisfactory; it cannot, however, on any account, dispense with a familiar knowledge of the Latin language, as constituting an essential part of a liberal education; at the commencement, therefore, of each oral examination, the candidate is called on to translate *viva voce* into Latin, a passage from Hippocrates, Galen, or Aretæus; or, if he declines this, he is, at any rate, expected to construe into English a portion of the works of Celsus, or Sydenham, or some other Latin medical author.

In connection with the oral examinations, the candidate is required, on three separate days, to give written answers in English to questions on the different subjects enumerated above, and to translate in writing passages from Greek or Latin books relating to medicine.

Those who are approved at all these examinations will receive the following diploma under the common seal of the College.

Sciant omnes, Nos, A. B., Præsidentem Collegii Medicorum Londinensis, unà cum consensù Sociorum ejusdem, auctoritate nobis a Domino Rege et Parliamento commissâ, examinâsse et approbâsse ornatissimum virum, T. S. et ei concessisse liberam facultatem et licentiam tam docendi quam exercendi scientiam et artem medicam, eidemque summis honoribus et titulis et privilegiis, quæcunque hic vel alibi Medicis concedi solent, intra auctoritatis nostræ limites frui dedisse. In cujus rei fidem et testimonium, adjectis Censorum et Registrarii chirographis, sigillum nostrum commune præsentibus apponi fecimus, datis ex ædibus Collegii die mensis anno Domini millesimo octingentesimo

Registrarius.

The College gives no particular rules as to the details of previous education, or the places at which it is to be obtained. It will be obvious, however, from a reference to the character and extent of the study above described, the manner in which the examinations are conducted, and the mature age of the candidates, as affording full time for acquiring the necessary knowledge, that there will be ample security afforded to the public and the profession, that none but those who have had a liberal and learned education can presume, with the slightest hope of success, to offer themselves for approval to the Censors' Board; and as the College trusts, that by a faithful discharge of its own duty, it can promise itself the satisfaction of thus continuing to admit into the order of English physicians, a body of men who shall do it honour by their qualifications, both general and professional, it is prepared to regard in the same light, and address by the same appellation, all who

have obtained its diploma, whether they have graduated elsewhere or not.

In drawing up and promulgating the above regulations, the College has endeavoured fairly to look at that which is substantial, rather than that which is merely nominal, in all that concerns the qualification of its members; it has resolved to estimate all testimonials, whether they are presented under the name of certificates, diplomas, or degrees, strictly with reference to their value, and to measure them by this standard alone, as parts of the previous qualification of candidates, which they are to verify in their examinations.

The College feels confident, that it has overstepped neither the spirit nor letter of the laws which have invested it with the power of governing and legislating for the whole Faculty of Medicine within its jurisdiction, by thus earnestly endeavouring to maintain its character and reputation, and vindicate its claim to be the source of professional honour.

FRANCIS HAWKINS, Registrar.

Dec. 22, 1838.

THE REVEREND MR. STEGGALL'S MEDICAL CARD.

LETTER FROM MR. STEGGALL.

WE have received the following letter from the Rev. Mr. Steggall. We accede to his request, and insert it in our columns, but we think that it is right that it should be printed *verbatim et literatim*. We are led to ask if the bishop of the diocese be acquainted with Mr. Steggall's proceedings? If we mistake not the Rev. Gentleman, in practising as an apothecary, is acting in direct opposition to the canons of the Church.

Sir:—A medical Practitioner (I presume) in the Neighbourhood of Stow Market, under the Assumed Name of Medicus has I find attacked me by a Paragraph in your Excellent Periodical, and in Vindication of my Respectability as a Clergeman and Medical Practitioner I am as it were compelled again to take the field and to request you to have the kindness to lay before your Numerous Medical Readers through the Medium of THE LANCET my Reasons for puting forth a Card to the Public somewhat Simulor to the One sent to you by Medicus for Insertion in the above mentioned Periodical for the 15th Dec. 1838. I had been Practiceing for two or three Years with great Success at Rottlesden and the Surrounding Neighbourhood unmolested When a Mr. White made his Appearance and took up his Residence within two Miles of the Parish where I am residing and opposed me in every way that he possibly could (not even bearing in Mind the Respectability of the Profession) but finding he could not Succeed in his professional Avocations so well as he was led to Anticipate he put forth to the Public a hand Bill which certainly is not so genteel a Method for a Medical Man as a Card more particularly for one who profess to be a Member of the Royal College of Surgeons.

The Particulars of his Hand Bill are as follows

W W Middleton White
Surgeon &c.

Having *fully Established himself during the Past Year* as a general Practitioner is induced from the peculiar state of opposition to him to Reduce his Charges as follows

Midwifery 15*s*., To the Labouring Class 7*s*. 6*d*.

Journeys 1*s*. the first Mile and sixpence for each Succeeding One

No Charges to the Labouring Class for Visits.

Medicines at Druggist's Price for Cash.

Gedding, Jan. 1st. 1837

After Such an Example set me by a Member of a Royal College of Surgeons I fancied I might Venture with Impunity (yet I very reluctantly did so) to adopt the same Unprofessional Method in pure opposition to Mr. White, and I put forth a Card under the Impression that it would confer a Benefit on the Small Trades People and the Labouring Class in the Neighbourhood. Now in perfect contradiction to the Statement of Medicus as to my resting my Title to Practise upon a few Months Residence with a Medical Man in the Country, I hereby assure him I do not but upon a much more legal and firm Basis, Actual Practice previous to 1815. I have now given to Medicus more Eplanation than a Medical Practitioner (who for anything I know to the contrary may be a Constant companion of Medicus) would favour me with when I put a few simple questions to him in THE LANCET for the 3rd Feb. 38 and likewise in the Ipswich Chronicle, none of which questions has he yet answered; should he think proper to do so Satisfactorily he may perhaps be favoured with a further Explanation of my Legal Right and Title to Practise as an apothecary, which perhaps will quiet his restless and Virulent Spirit which Vitiates the Heart where it dwells and cause the possessed of such a Demoniacal Visitant to Vent his Spleen upon Unoffending Objects. Medicus I find Wishes to be informed if there is not a Law to Punish such Practitioners as he Invidiously Terms Me. Surely I have more reason inquire is there no Law to Punish the Vile and Dastardly Calumniator who is afraid and ashamed to affix his Name to his Ungentlemanly and Malignant Publications. Your having the kindness

to permit the foregoing Letter to be Inserted in your forth coming Number of THE LANCET will confer an Obligation on

Your Most Respectful
And Obedient Servant,

J. H. STEGGALL.

Rottlesden Vale, near Woolpit,
Jan. 3, 1839.

EFFECT OF THE

ALKALINE CARBONATES ON THE BLOOD.

To the Editor of THE LANCET.

SIR:—Some of your correspondents have very vehemently denounced the *alkaliphobia* of M. Magendie, apparently under the impression that he stands alone in his opinions respecting the action of the alkaline carbonates on the blood; but the truth is, that not only is he not the sole believer in the doctrine he lays down, but that he has actually been anticipated in it by divers experimenters and practical physicians. This the following passages in Dr. Copland's Dictionary will fully prove:—

"The excessive or long-continued use of alkalies, or of alkaline salts with excess of base, has the effect of diminishing the cohesion and the viscosity of the blood, and of preventing it from coagulating after it has been removed from the vessels; and while these substances thus, as it were, dissolve, or attenuate this fluid, they also diminish the vital cohesion and tonic contractility of the extreme vessels and of the tissues, and create a disposition to extravasation of blood in the parenchyma of the organs and to exudation of it from the mucous surfaces."—Part I., p. 187.

And again:—"The attenuating effects of the fixed and volatile alkalies, and of their subcarbonates, both upon the blood and the secretions formed from it, particularly when long employed, will be considered as proved by any one who will peruse the experiments of Schwencke, Friend, Eller, Rutty, Courten, Pitcairne, Thackrah, and Scudamore, without the bias of system; and they are confirmed in my mind by some observations I have made of the results when those substances are mixed with blood immediately after venesection, or when exhibited internally in large doses for some time previous to abstraction of the blood."—P. 192.

Will you give this little rectification of what appears to be a pretty general error, a corner in the next number of your valuable Journal, and oblige yours,

SUUM CUIQUE.

Jan. 5, 1839.

THE APOTHECARIES' COMPANY OF LIVERPOOL.

To the Editor of THE LANCET.

SIR:—I beg to call your attention, as the censor of the medical profession, to the proceedings of some of its members in the important town of Liverpool. A number of the physicians, surgeons, and apothecaries of that place have united in a joint-stock company, with a host of unqualified individuals, and have, in imitation of the worthies at Rhubarb Hall, appropriated to themselves the specious title of "The Apothecaries' Company." This scheme has been started under the pretext of an anxiety for the dispensing of genuine drugs and securing the public against adulterations. Several malpractices, I am informed, can already be proved against them, from which it would appear that the project is either a mere commercial speculation, without the wholesome check of personal responsibility, or that they and their managers are ignorant of the characters and qualities of drugs, and, therefore, incompetent to carry their proposed object into effect.

But a more glaring charge against this extraordinary Association is, that they are become wholesale and retail venders of all kinds of advertised nostrums, so that Godfrey's cordial, Morison's pills, Hunt's family ditto, and any other such articles, may be purchased from the firm of "Doctors Carson, Brandreth, O'Donnell, Bryce, and Co.;" and, Sir, these are the men whose names stand at the head of a requisition to the Provincial Association, begging that worthy conclave to honour the town of Liverpool with their next meeting; but we need not wonder, for such tallies well with the "make believe" disposition of this provincial society, who, while they appoint a section to provide against the inroads of quackery, permit their very supporters to become absolute agents against them.

The expression of your sentiments on what appears to me, and to many of my brethren in this town, to be so derogatory to the character of the profession, will much oblige yours, respectfully,

MACHAON.

Manchester, Jan. 1, 1839.

CORRESPONDENTS.

SEVERAL letters and communications, for which we have been unable to find room this week, shall appear in our next Number.

ERRATA.—In the last number of THE LANCET, page 557, col. 2, line 1, *for* 647 *read* 653. Page 558, col. 1, line 10, *for* 502 *read* 462; and line 11, *for* 22 *read* 92; col. 2, line 12, of same page, *for* attacked *read* cut off.